PHILOSOPHICAL

TRANSACTIONS

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCXXVI.

PART II.

LONDON:

PRINTED BY W. NICOL, SUCCESSOR TO W. BULMER AND CO.
CLEVELAND-ROW, ST. JAMES'S;
AND SOLD BY G. AND W. NICOL, PALL-MALL, PRINTERS TO THE
ROYAL SOCIETY.

MDCCCXXVI.

ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March, 1752. And the grounds of their choice are, and will continue to

be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they received them, are to be considered in no other light than as a matter of civility, in return for the respect shown to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

CONTENTS.

I. *An Account of the construction and adjustment of the new Standards of Weights and Measures of the United Kingdom of Great Britain and Ireland.* By Captain HENRY KATER, *F. R. S.* - - - - page 1

II. *Description of an improved Hygrometer.* By *Mr.* THOMAS JONES. *Communicated by Captain* HENRY KATER, *F. R. S.* 53

III. *Observations on the changes which have taken place in some antient alloys of copper.* By JOHN DAVY, *M. D. F. R. S. In a Letter to Sir* HUMPHRY DAVY, *Bart. Pres. R. S.* - 55

IV. *Additional proofs of animal heat being influenced by the nerves.* By *Sir* EVERARD HOME, *Bart. V. P. R. S.* 60

V. *The Croonian Lecture. On the structure of a muscular fibre from which are derived its elongation and contraction.* By *Sir* EVERARD HOME, *Bart. V. P. R. S.* - - 64

VI. *An account of the heat of July, 1825; together with some remarks upon sensible cold.* By W. HEBERDEN, *M. D. F. R. S.* - - - - - 69

VII. *On the transit instrument of the Cambridge Observatory; being a Supplement to a former Paper.* By ROBERT WOODHOUSE, *Esq. Plumian Professor of Astronomy in the University of Cambridge,* - - - - 75

VIII. *Account of a series of observations, made in the summer of the year 1825, for the purpose of determining the difference of meridians of the Royal Observatories of Greenwich and Paris; drawn up by* J. F. W. HERSCHEL, *Esq. M. A. Sec. R. S. Communicated by the Board of Longitude,* - - 77

IX. *Observations on the poison of the common toad.* By JOHN DAVY, *M. D. F. R. S.* - - - page 127

X. *On the magnetizing power of the more refrangible solar rays.* By *Mrs.* M. SOMERVILLE. *Communicated by* W. SOMERVILLE, *M. D. F. R. S. Feb.* 2. 1826, - - 132

XI. *On the mutual action of sulphuric acid and naphthaline, and on a new acid produced.* By M. FARADAY, *F. R. S. Corresponding Member of the Royal Academy of Sciences, &c. &c. Communicated January* 12, 1826, - - - 140

XII. *On the nervous circle which connects the voluntary muscles with the brain.* By CHARLES BELL, *Esq. Communicated by the President, January* 25, 1826, - - - 163

XIII. *On the constitution of the atmosphere.* By JOHN DALTON, *Esq. F. R. S.* - - - - - - 174

PHILOSOPHICAL TRANSACTIONS.

I. *An Account of the construction and adjustment of the new Standards of Weights and Measures of the United Kingdom of Great Britain and Ireland.* *By Captain* HENRY KATER, *F. R. S.*

Read November 24, 1825.

THE weights and measures of the United Kingdom of Great Britain and Ireland are founded upon a standard, the length of which is determined by the proportion it bears to that of the pendulum vibrating seconds of mean time in London. The length of this pendulum I have stated to be 39,13929 inches of Sir GEORGE SHUCKBURGH EVELYN's standard scale; and the experiments from which this conclusion is deduced, are detailed in the Philosophical Transactions for 1818. The important consequences, however, which attach to this result, render it necessary to examine with what degree of confidence it may be received.

In the Paper to which I have alluded, it is mentioned that a series of experiments had been made previous to those detailed; the result of which was rejected, in consequence of its being discovered, after their completion, that the steel

plates, on which the pendulum had rested during the experiments, had suffered penetration by the knife edges. The length of the seconds pendulum however, deduced from these first experiments, did not differ more than two ten-thousandths of an inch from the final determination.

It may also be seen, that in repairing the knife edges after the first set of experiments had been concluded, one of them was broken; and when replaced by another, the distance between the knife edges was increased about one hundredth of an inch; so that two results, differing by so small a quantity from each other, may be considered as having been obtained by two different instruments.

The Philosophical Transactions for 1819 contain an account of experiments for determining the variation in length of the seconds pendulum at the principal stations of the Trigonometrical Survey of Great Britain. For this purpose I constructed an invariable pendulum, the number of vibrations made by which being observed in London, and also at the principal stations of the Trigonometrical Survey, the length of the seconds pendulum, at such stations, could readily be deduced from the previously known length of the pendulum vibrating seconds in London.

In this manner, the length of the pendulum vibrating seconds at Unst was found to be 39,17146 inches, and at Leith Fort 39,15554 inches.

The "*Recueil d'Observations Geodesiques*," &c. which may be considered as a fourth volume of the "*Base du Systéme métrique*," affords me an opportunity of comparing these last results with those of M. BIOT, whose very valuable labours for determining the length of the pendulum vibrating

seconds at various stations, from Unst to Formentara, are there detailed. In these experiments M. Biot employed the method of Borda, which requires that the *absolute* length of the pendulum should be obtained by actual measurement at each station. M. Biot's observations and mine, both at Unst and Leith Fort, were made at the same stations; and M. Biot found, from the mean of fifty-six series, using different measuring rods and various pendulums, the length of the second pendulum at Unst to be ,994943083 metres, and at Leith Fort ,994524453 metres.

Converting these results into inches of Sir George Shuckburgh's scale, by taking the length of the metre at 39,37079 inches, as given in the Philosophical Transactions for 1818, we have the length of the seconds pendulum at Unst, after reduction to the level of the sea, 39,17176 inches, and at Leith Fort 39,15539 inches, the first differing from my determination +,00029, and the latter −,00015 of an inch. The difference of results obtained by methods totally dissimilar being so small, and with contrary signs, it may be reasonably inferred from them, as well as from what has been before advanced, that the length of the pendulum vibrating seconds in London has been determined to within one ten-thousandth of an inch of the truth.

From the near agreement of the results of the French and English experiments on the length of the pendulum, it may be inferred that the length of the French metre, expressed in parts of Sir George Shuckburgh's scale, is probably not erroneous one ten thousandth of an inch.

From an account recently published by Captain Sabine, F. R. S. of his valuable experiments for determining the vari-

ations in length of the seconds' pendulum, doubts may be inferred of the accuracy of the method which I employed in the observations for ascertaining the length of the seconds' pendulum in London, as well as in those which have been made with the invariable pendulum. It is asserted, that taking a mean between the disappearance and re-appearance of the disk, is a more correct method of observation than that which I have pursued; and that the intervals between the coincidences obtained by observing the disappearances only of the disk, would be productive of error.

With respect to the convertible pendulum, it will be seen on referring to the Philosophical Transactions for 1818, that the disk was made to subtend precisely the same angle as the tail-piece of the pendulum; so that at the moment of disappearance of the last portion of the disk, its centre coincided with the middle of the tail-piece, a circumstance which, in my method of observing is indispensable, when the object is to obtain the *true* number of vibrations made by the pendulum in twenty-four hours.

With the invariable pendulum, from causes unnecessary here to detail, the circumstances were somewhat different, the disk subtending a *less* angle than that of the tail-piece of the pendulum; in consequence of which, the interval between the apparent coincidences was lessened, and the inferred number of vibrations in twenty-four hours diminished about two-tenths of a vibration; but as the experiments with the invariable pendulum are intended to be merely comparative, and should therefore be made as nearly as possible in every respect under similar circumstances, no part of the apparatus being changed, nor any alteration made in the pendulum of

the clock, this constant difference of the number of vibrations from the truth will not affect the ultimate deductions. As the most direct method of removing any doubts which may be entertained upon this subject, I shall add a comparative Table of the successive differences of the number of vibrations in twenty-four hours, which I have computed from the whole of Captain SABINE's observations, by employing the disappearances only, of the disk.

Stations.	Differences by the method of disappearances.	Differences by the method of disappearances and re-appearances.	Difference of the results.
	Vibrations.	Vibrations.	Vibrations.
St. Thomas . . .	9,62	9,62	0,00
Maranham . . .	13,29	13,33	— 0,04
Ascension	4,97	4,97	0,00
Sierra Leone . . .	0,85	0,83	+ 0,02
Trinidad	5,51	5,50	+ 0,01
Bahia	12,40	12,46	— 0,06
Jamaica	72,72	72,71	+ 0,01
New York . . .	41,88	41,81	+ 0,07
London	38,62	38,73	— 0,11
Drontheim . . .	22,48	22,44	+ 0,04
Hammerfest . . .	9,53	9,48	+ 0,05
Greenland	12,62	12,49	+ 0,01
Spitzbergen . . .			
		Mean . .	0,00

As the comparative results, by the method of disappearances, would to a certain degree be vitiated by the use of a disk of a different size, the observations made in London in 1824 with a silver disk are omitted.

The differences of the results being so minute, and the mean of the whole being absolutely nothing, the conclusion is warranted that the method of observing coincidences by

disappearances alone is productive of no error, if the observations are made as nearly as may be under similar circumstances.

With respect to the absolute length of the pendulum in London, as determined by means of the convertible pendulum, it must be evident from what has been advanced, that the method of observation by disappearances alone could, on that occasion, have been productive of no error, as the disk subtended precisely the same angle as the tail-piece of the pendulum.

In the Philosophical Transactions for 1821 will be found "An account of the comparison of various British Standards of linear measure;" and it will there be seen that the difference between Sir GEORGE SHUCKBURGH'S standard scale and a standard yard of 1760, made by BIRD, and in the custody of the Clerk of the House of Commons, is so very small, that they may be considered as "perfectly identical." This yard, under the denomination of "the Imperial Standard Yard," has been declared by Parliament, on the recommendation of the Commissioners of Weights and Measures,* to be the "unit, or only standard measure of extension" of the United Kingdom; consequently, the length of the pendulum before given is expressed in parts of the Imperial Standard Yard.

The measures of capacity being dependant upon the weight of a cubic inch of distilled water, it became necessary to

* A commission was appointed by letters patent under the Great Seal of the United Kingdom in 1818, "For considering how far it might be practicable and advisable to establish a more uniform system of Weights and Measures," the members of which were, the late Sir Joseph Banks, Sir George Clerk, Mr. Davies Gilbert, Dr. W. H. Wollaston, Dr. Thomas Young, and Capt. Henry Kater.

consider the degree of reliance which might be placed upon the accuracy of the experiments made by Sir GEORGE SHUCKBURGH on this subject.

A cube, a cylinder, and a sphere of brass were used in these experiments; and in consequence of some difficulties which occurred, the weight of these, both in air and in distilled water, was so repeatedly ascertained by Sir GEORGE SHUCKBURGH, as to leave no reasonable doubt of the accuracy of his conclusions. But as the linear dimensions of the cube, sphere and cylinder, might not have been determined with equal precision, it was thought adviseable to repeat these measurements. This was accordingly done; and though in detail they differed somewhat from Sir GEORGE SHUCKBURGH's determinations, yet, fortunately, the variations were such as nearly to counterbalance each other, and to occasion little difference in the ultimate result, as may be seen in an account of these last mentioned measurements given in the Philosophical Transactions for 1821.

Sir GEORGE SHUCKBURGH's results having been expressed in terms derived from the mean of several standard Troy weights, of one and of two pounds, kept at the House of Commons, the Commissioners of Weights and Measures thought it desirable that the Troy pound, which differed the least from this mean, should be taken as the unit of weight; and therefore recommended that the Parliamentary Standard Troy pound of 1758 should remain unaltered; and this has been accordingly declared by the Legislature to be the original "unit, or only standard measure of weight, from which all other weights shall be derived;" and that it is to be denominated "the Imperial Standard Troy Pound."

From Sir GEORGE SHUCKBURGH'S data, and the before-mentioned measurements of the cube, cylinder and sphere, the Commissioners of Weights and Measures determined the weight of a cubic inch of distilled water, weighed in air by brass weights at the temperature of 62° of Fahrenheit's thermometer, the barometer being at 30 inches, to be equal to two hundred and fifty-two grains, and four hundred and fifty-eight thousandth parts of a grain, of which the Imperial Standard Troy pound contains five thousand seven hundred and sixty; and also that the avoirdupois pound, which has long been in general use, though not established by any act of the Legislature, is so nearly seven thousand grains, that they recommended that seven thousand of such troy grains be declared to constitute a pound avoirdupois.*

The Commissioners of Weights and Measures also recommended that the gallon measure should in future be that which contains ten pounds avoirdupois of water in ordinary circumstances (that is to say, the temperature of the water being 62° of Fahrenheit's thermometer, and the barometer 30 inches); and this, under the denomination of "the Imperial Standard Gallon," was declared to be the unit and only standard measure of capacity, and that eight such gallons should be a Bushel.

The Act for "ascertaining and establishing uniformity of "Weights and Measures" having been passed by Parliament, to the effect before-mentioned, the Lords Commissioners of His Majesty's Treasury expressed their hope, that some of

* For the manner in which the weight of a cubic inch of distilled water was deduced, see the Appendix to the Third Report of the Commissioners of Weights and Measures.

the Commissioners of Weights and Measures would afford their assistance in directing the executive measures necessary for the accomplishment of its objects. In compliance with this request, I undertook to superintend the construction, and to adjust the principal standards to be deposited at the Exchequer, Guildhall, Dublin, and Edinburgh.

Mr. TROUGHTON, the first of our English artists having, in consequence of his advanced age, declined undertaking to make the standards, I directed Mr. DOLLOND to prepare those of linear measure; and employed Mr. BATE, an artist who, as hydrometer maker for Revenue purposes, had been accustomed to nice operations in weighing, to construct those of weight and measure.

Brass being peculiarly liable to decomposition in the atmosphere of London, I directed Mr. BATE to make some experiments, to ascertain the proportions of tin and copper which might produce a metal equal in hardness, and which might be worked with the same facility as hammered brass; and after some trials it was found that a mixture of 576 parts of copper, 59 of tin, and 48 of brass, afforded a beautiful metal, which possessed the qualities I sought.

In order to avoid any innovation but such as might be absolutely necessary, it was deemed expedient in constructing the bushel, to adhere as nearly as possible to the form of that known by the appellation of the Winchester bushel. It was therefore directed to be made cylindrical, the interior diameter being about $18\frac{1}{2}$ inches, the exterior $19\frac{1}{2}$ inches, and the depth about $8\frac{1}{4}$ inches, and intended to contain eighty pounds avoirdupois of distilled water. In order to give the bushel

additional strength, it was cast with two projecting hoops, one to which the bottom was screwed, and another at the distance of about half an inch from the top.

Considerable difficulties arose in casting the bushel; out of twelve, only five proved sound enough for use; but by varying the process, they were at length procured sufficiently perfect. Much credit is due to Mr. KEIR, the engineer employed by Mr. BATE in turning the bushels, for the beauty and perfection of his work.

The form of the gallon measure occupied much of my attention. It was necessary that it should be such as to enable me to determine the weight of distilled water it should contain with the least liability to error. The conical form was therefore adopted; the mouth being made cylindrical, and one and a half inches diameter: the top was ground perfectly flat, and the edge so rounded off, that the contents might be poured from it into any other vessel without running down the side. The cone was placed in a cylinder about four inches high, in which handles were formed, and which served at the same time to protect the gallon from injury, and to prevent any change of temperature which might arise from handling. The quart and the pint measures were of the same form on a smaller scale.

The weights were of brass, and nearly of a spherical form, but flattened at the bottom. Into the top was screwed a button; beneath which a small cavity was left to receive such minute pieces of wire as might be found requisite to make up the standard weight. This button served also to lift the weight by means of a strong wooden fork.

The Imperial Troy pound is in the custody of G. WHITTAM, Esq. Clerk to the House of Commons, who obligingly permitted me to compare two weights with the standard at his house in Abingdon-street. These being intended merely as rough models for Mr. BATE to copy, it was not thought necessary to waste time in adjusting them with the utmost precision, and they were left a little in excess.

The five Troy pounds being completed by Mr. BATE from the copy of the Imperial pound taken at Mr. WHITTAM'S, I proceeded to compare them with the Imperial Troy pound, which was then confided to my care for that purpose. The balance employed in making the comparisons was made by Mr. ROBINSON, an artist who has directed much of his attention, and with great success, to the improvement of this instrument. The beam is only ten inches long, and together with the scale pans weighs eight hundred grains. The scale pans are attached to agate planes, which rest upon knife edges. The ultimate angle of the central knife edge is 120 degrees. This balance, with one pound avoirdupois in each scale, it may be seen by the following details, varies immediately one division, equal to about one-tenth of an inch, on the addition of one-hundreth of a grain.

Adjustment of the standard troy pounds.

In order to distinguish the troy pound weights from each other, they were marked in the following manner:

That which I shall call
No. 1. T^Y P^D / 1824
No. 2. T^Y P^D / 1824· a dot after the figures.
No. 3. T^Y P^D: a dot under the letter D. / 1824
No. 4. T^Y P^D: a dot under the letter D, and another / 1824· dot after the figures.
No. 5. T:Y P^D: a dot under the letter Y, another dot under the letter D, and a third dot / 1824· after the figures.

The Imperial Standard troy pound was put into the scale, and counterpoised. The division and fraction of a division which would have been pointed out by the index, had the beam been allowed to come to a state of rest, was ascertained by taking the mean of the extremes of the vibrations, when their extent did not exceed one division. By this much time was saved, and it is presumed greater accuracy attained. The standard pound was then removed and replaced by the copy, and the point of rest determined as before: the difference between these means converted into parts of a grain, gave the error of the copy. The same method was followed in the adjustment of all the weights.

No. 1. being too heavy, was laid aside for the present.

No. 3. being too light for the limits of the index, was also set aside.

By many careful trials, one-hundredth of a grain occasioned a variation in the index of 0,67 divisions.

Standard Pound Divisions.	No. 2. Divisions.	Error Divisions.	Mean.	Error in grains.
+ 0,5	— 2,0	— 2,5	— 2,5	— ,037
+ 1,0	— 1,5	— 2,5		
+ 1,0	— 1,5	— 2,5		
	No. 4.			
+ 1,4	+ 0,9	— 0,5	— 0,15	— ,0022
+ 0,1	+ 0,0	— 0,1		
+ 0,3	+ 0,4	+ 0,1		
+ 0,4	+ 0,3	— 0,1		
	No. 5.			
+ 0,5	+ 0,1	— 0,4	— 0,3	— ,0044
+ 0,0	— 0,1	— 0,1		
+ 0,4	+ 0,3	— 0,1		
+ 0,2	+ 0,3	— 0,5		
+ 0,1	— 0,3	— 0,4		

Wires of the following weights were now added:

To No. 2. - - ,020 grains.
3. - - ,050
4. - - ,002
5. - - ,004

The centre of gravity of the balance was raised, so that by frequent trials, one hundredth of a grain occasioned the index to vary one division.

Standard Pound Divisions.	No. 2. Divisions.	Error Divisions.	Mean.	Error in grains.
— 0,5	— 0,1	+ 0,4	+ 0,43	— ,0043
— 0,6	+ 0,3	+ 0,9		
— 0,2	— 0,6	— 0,4		
+ 0,1	+ 0,1	+ 0,0		
— 0,2	— 2,1	— 1,9		
— 0,5	— 2,1	— 1,6		

The centre of gravity was lowered, so that one-hundredth of a grain was equal to eight-tenths of a division.

Standard Pound Divisions.	No. 3. Divisions.	Error Divisions.	Mean.	Error in grains.
— 0,5	— 0,8	— 0,3	— 0,52	— ,0065
0,0	— 0,9	— 0,9		
— 0,3	— 0,8	— 0,5		
— 0,3	— 0,7	— 0,4		
	No. 4.			
+ 0,3	+ 0,3	0,0	— 0,17	— ,0021
+ 0,1	+ 0,1	0,0		
+ 0,5	— 0,2	— 0,7		
— 0,1	0,0	+ 0,1		
0,0	— 0,4	— 0,4		
— 0,1	— 0,1	0,0		
	No. 5.			
— 0,1	— 0,2	— 0,1	— 0,08	— ,0010
+ 0	0,0	— 0,1		
+ 0,1	+ 0,2	+ 0,1		
+ 0,2	+ 0,2	0,0		
+ 0,2	+ 0,1	— 0,1		
+ 0,3	— 0,2	— 0,5		
— 0,2	— 0,1	+ 0,1		

The following quantities were added to the different weights:

To No. 3. - - ,005 grains.
4. - - ,002
5. - - ,002

Mr. ROBINSON exchanged the balance I had hitherto used for another, which he thought superior. The mean of many trials gave 0,7 of a division for the variation occasioned by one-hundredth of a grain.

Standard Pound Divisions.	No. 2. Divisions.	Error Divisions.	Mean.	Error in grains.
0,0	+ 0,3	+ 0,3		
+ 0,1	+ 0,4	+ 0,3		
+ 0,3	+ 0,2	— 0,1	+ 0,2	+ ,0029
+ 0,1	+ 0,3	+ 0,2		
— 0,1	+ 0,2	+ 0,3		
+ 0,1	+ 0,4	+ 0,3		
	No. 3.			
— 0,4	— 1,0	— 0,6		
— 0,9	— 1,0	— 0,1	— 0,52	— ,0074
— 0,8	— 1,4	— 0,6		
— 1,5	— 2,3	— 0,8		
	No. 4.			
+ 0,1	+ 0,5	+ 0,4		
+ 0,1	0,0	— 0,1		
0,0	+ 0,1	+ 0,1		
0,0	+ 0,8	+ 0,8		
+ 0,7	+ 1,0	+ 0,3	+ 0,29	+ ,0041
+ 0,7	+ 0,8	+ 0,1		
+ 0,6	+ 0,8	+ 0,2		
+ 0,7	+ 1,2	+ 0,5		
	No. 5.			
+ 0,1	+ 0,1	0,0		
0,0	0,0	0,0		
0,0	0,0	0,0	0,0	0,0
0,0	— 0,1	— 0,1		

By many careful trials previous to the following comparisons, one-hundredth of a grain occasioned a variation of 0,9 of a division.

Standard Pound Divisions.	No. 2. Divisions.	Error Divisions.	Mean.	Error in grains.
0,0	+ 0,1	+ 0,1		
0,0	0,0	0,0		
— 0,2	+ 0,1	+ 0,3	+ 0,16	+ ,0018
+ 0,15	+ 0,3	— 0,15		
— 0,05	+ 0,35	+ 0,4		
0,0	+ 0,3	+ 0,3		

No. 1. was reduced in weight, and being then too light, ,005 of a grain was enclosed in it and the following comparisons made:

Standard Pound Divisions.	No. 1. Divisions.	Error Divisions.	Mean.	Error in grains.
— 0,2	— 0,15	+ 0,05		
— 0,15	— 0,35	— 0,20		
— 0,35	— 0,50	— 0,15	+ 0,09	+,001
— 0,5	— 0,20	+ 0,30		
+ 0,15	+ 0,70	+ 0,55		
+ 0,1	— 0,10	0,00		

Added ,005 of a grain to No. 3.

Standard Pound Divisions.	No. 1. Divisions.	Error Divisions.	Mean.	Error in grains.
— 0,4	— 0,65	— 0,25		
— 0,6	— 0,50	+ 0,10		
— 0,4	— 0,40	0,0	0,0	,00
— 0,1	— 0,20	— 0,10		
+ 0,1	+ 0,20	+ 0,10		
+ 0,1	+ 0,20	+ 0,10		
	No. 3.			
+ 0,1	+ 0,15	+ 0,05		
0,0	0,0	0,0		
0,0	0,0	0,0	+ 0,04	+,0005
— 0,1	0,0	+ 0,1		
— 0,1	— 0,2	— 0,1		
0,0	+ 0,2	+ 0,2		

The wires which were found necessary to make each of the copies equal to the Standard troy pound being enclosed, the button was screwed in with a force sufficient to resist any ordinary attempt to detach it; but should such an endeavour ever be made with success and the wires be lost, the following table contains an account of their number and value.

No. 1. contains 1 wire = ,005 gr.
2. . . 1 wire = ,020 gr.
3. . . 3 wires = ,050 + ,005 + ,005 gr.
4. . . 2 wires = ,002 + ,002 gr.
5. . . 2 wires = ,004 + ,002 gr.

In order to employ the whole of the preceding comparisons in deducing a final result, it will be necessary to add to each error given in the Tables the sum of the weights which were enclosed after the determination of such error.

Troy Pound No. 1.

	Grains.
The mean of six comparisons gave for the error of No. 1. - - - -	+,001
The mean of six other comparisons - -	,000

Troy Pound No. 2.

The mean of three comparisons gave for the error of No. 2. —,037 + ,02 =	—,0170
The mean of six other comparisons -	+,0029
of six other comparisons -	+,0018

Troy Pound No. 3.

The mean of six comparisons gave —,0048 + ,005 + ,005 = - -	+,0057
The mean of four other comparisons gave —,0065 + ,005 + ,005 = -	+,0035
The mean of four other comparisons gave —,0074 + ,005 - - = -	—,0024
The mean of six other comparisons - -	+,0005

Troy Pound No. 4.

	Grains.
The mean of four comparisons gave	
—,0022 + ,004 = - - - -	+ ,0018
The mean of six other comparisons gave	
—,0021 + ,002 = - - - -	—,0001
The mean of eight other comparisons -	+ ,0041

Troy Pound No. 5.

The mean of five comparisons gave	
—,0044 + ,006 = - - -	+ ,0016
The mean of seven other comparisons gave	
—,001 + ,002 = - - -	+ ,0010
The mean of four other comparisons - -	,0000

In order to deduce a final mean from these results, each result was multiplied by the corresponding number of comparisons, and the sum of the products thus obtained was divided by the sum of the comparisons. In this manner the following errors of each of the troy pounds was determined.

	Grains.
No. 1. - -	+ ,0005
2. - -	—,0015
3. - -	+ ,0021
4. - -	+ ,0029
5. - -	+ ,0010

These errors being so small and mostly in excess, it was not thought necessary to make any attempt to correct them.

The five troy pounds previous to their being delivered to

me had been adjusted by Mr. BATE, and with the exception of an excess of one hundredth of a grain common to all, and which appears to have been the error of the copy taken at Mr. WHITTAM's, the greatest difference from the preceding results did not exceed four or five-thousandths of a grain.

The adjustment of the five troy pounds occupied me from the commencement till the middle of March 1825, during which time Mr. BATE was engaged in preparing a balance sufficiently strong to be used in determining the gallon weights; this balance was brought to my house on the 16th of March.

I shall now proceed to describe the manner in which the avoirdupois pound was derived from the troy pound.

For this purpose Mr. BATE made the following weights:

1	of	5760	grains.
2	—	2880	
3	—	960	
2	—	480	
2	—	240	
5	—	48	
2	—	24	
2	—	12	
2	—	6	
2	—	3	
3	—	1	

In all twenty-six weights.

Of the above weights the following were used to make up an avoirdupois pound:

5760
960
240
24
12
3
1
7000 grains.

In this manner five avoirdupois pounds were made and adjusted by Mr. BATE.

Determination of the Avoirdupois Pound.

Wishing to avoid the verification of the numerous weights used by Mr. BATE, and desirous of arriving at my object by a road different from that which he had pursued, I used the following method:

I caused Mr. BATE to make two weights each equal to six troy pounds. These I compared with the Imperial Standard Troy pound, and the five troy pounds already adjusted. I had thus two known weights each equal or nearly so to six troy pounds, one of which with the addition of 440 grains gave me a weight equal to five avoirdupois pounds, subject only to such error as might arise from inaccuracy in the grains, and as I had had sufficient proof of Mr. BATE's care, I had little to apprehend on this point.

The five avoirdupois pounds were then compared with each of the six pound weights and 440 grains, and the mean

taken, and thus the sum of the errors of the five avoirdupois pounds became known. Assuming one of these pounds (No. 1.) as a standard, each of the others was then compared with it, and their relative errors thus ascertained, from which, and the sum of the errors of the five avoirdupois pounds, the error of each was deduced.

The following is a detail of the comparisons.

The variation of the index was carefully ascertained to be one division for one-tenth of a grain.

The six Troy Pounds, Divisions.	The 6 lb. weight, No. 1. Divisions.	Difference Divisions.	Mean.	Difference in grains.
−0,4	−2,0	−1,6	−2,9	−0,29
+1,1	−1,0	−2,1		
+1,0	−4,0	−5,0		
−1,9	−5,0	−3,1		
Added three-tenths of a grain to the 6 lb. weight No. 1.				
−1,8	−0,5	+1,3	+0,2	+0,02
−2,9	−4,8	−1,9		
−5,7	−3,8	+1,9		
−1,7	−2,3	−0,6		
−3,4	−2,7	+0,7		
−2,5	−2,6	−0,1		

By several trials one-tenth of a grain occasioned a variation in the index of 0,7 of a division.

The six Troy Pounds, Divisions.	The 6 lb. weight, No. 2. Divisions.	Difference Divisions.	Mean.	Difference in grains.
0,0	— 2,0	— 2,0	— 2,2	— 0,31
— 1,0	— 4,8	— 3,8		
— 3,6	— 5,2	— 1,6		
— 2,2	— 3,6	— 1,4		
Added two-tenths of a grain to the 6 lb. weight No. 2.				
+ 1,5	— 0,5	— 2,0	— 1,54	— 0,22
+ 1,2	— 1,5	— 2,7		
+ 1,7	+ 1,2	— 0,5		
— 3,8	— 1,8	+ 2 0		
— 3,8	— 7,0	— 3,2		
— 4,8	— 4,6	+ 0,2		
— 0,2	— 4,8	— 4,6		
— 2,4	— 3,6	— 1,2		
— 2,3	— 3,6	— 1,		
— 1,9	— 4,6	— 2,7		
— 3,3	— 4,3	— 1,0		

Added 0,17 of a grain.

The 6 lb. weight No. 1.

The mean of four comparisons gave for the difference of No. 1. from the six troy pounds, $-,29+,3=+,01$

The mean of six other comparisons - - $+,02$

The 6 lb. weight No. 2.

The mean of four comparisons gave for the difference of No. 2. from the six troy pounds $-,31+,2+,17=+,06$

The mean of 11 other comparisons gave $-,22+,17=-,05$

The final means of these results obtained in the same manner as those of the troy pounds are as follows:

	Grains.
The difference of No. 1, from the six troy pounds is	$+,016$
of No. 2, - - - - -	$-,020$
The mean difference is	$-,002$

The avoirdupois pounds were distinguished from each other by the following marks.

That which I shall call
No. 1. 1 P^{D} Av^{S} 1824

No. 2. 1 P^{D} Av^{S} 1824· a dot after the figures.

No. 3. 1 P^{D} Av$^{\cdot S}$ 1824· a dot under the letter S, and another dot after the figures.

No. 4. 1 P$^{D}_{\cdot}$ Av$^{\cdot S}$ 1824· a dot under the letter D, another dot under the letter S, and a third dot after the figures.

No. 5.

These weights were successively compared with the avoirdupois pound No. 1. which was taken both before and after the others; for this purpose Mr. ROBINSON's balance was used.

One hundredth of a grain occasioned a variation of 0,7 of a division.

Avoirdupois Pounds.						
No. 1. Divisions.	No. 2. Divisions.	No. 3. Divisions.	No. 4. Divisions.	No. 5. Divisions.	No. 1. Divisions.	
+ 0,2	— 0,4	— 0,4	— 0,8	— 0,9	+ 0,3	
— 0,4	— 0,4	— 0,4	— 0,7	— 0,8	— 0,2	
— 0,4	— 0,3	— 0,4	— 0,7	— 0,8	— 0,5	
— 0,5	— 0,4	— 0,3	— 0,7	— 0,7	— 0,2	
— 0,28	— 0,37	— 0,37	— 0,72	— 0,8	— 0,15	Means.
— 0,15						
— 0,21	Mean.					

By the above comparisons the difference of each weight from No. 1. is as follows:

No. 2.	— ,16 divisions	=	— ,002 grains.
3.	— ,16	=	— ,002
4.	— ,51	=	— ,007
5.	— ,59	=	— ,008
	Sum of the differences		— ,019

I now proceeded to compare the five avoirdupois pounds with each of the 6lb. weights, to which 440 grains, taken from Mr. BATE's weights, were added, and which together made five pounds avoirdupois.

One tenth of a grain occasioned the index to vary one division.

The 6 lb. weight No. 1. + 440 gr. Divis.	The five Avoirdupois Pounds, Divisions.	Difference Divisions.	Mean.	Difference in grains.
— 1,0	— 3,9	— 2,9		
— 0,8	— 0,5	+ 0,3		
+ 0,6	+ 2,2	+ 1,6		
+ 1,8	+ 1,1	— 0,7		
+ 1,7	+ 0,1	— 1,6		
+ 1,5	+ 1,1	— 0,4	— 0,63	— 0,063
0,0	— 0,4	— 0,4		
+ 2,7	— 0,7	— 3,0		
— 1,9	— 2,9	— 1,0		
— 2,2	— 1,4	+ 0,8		

The 6 lb. weight No. 2. + 440 gr. Divis.	The five Avoirdupois Pounds, Divisions.	Difference Divisions.	Mean.	Difference in grains.
— 1,0	— 1,3	— 0,3		
3,0	— 0,7	+ 2,3		
— 1,6	— 2,1	— 0,5		
— 1,2	— 1,2	0,0		
— 0,9	— 0,8	+ 0,1		
— 2,1	— 0,7	+ 1,4		
— 1,6	— 1,2	+ 0,4	+ 0,26	+ 0,026
— 0,4	— 1,0	— 0,6		
— 0,2	— 0,7	— 0,5		
— 0,7	— 0,4	+ 0,3		

Deductions from the preceding comparisons.

The mean of the differences of the two 6 lb. weights from the six troy pounds is - - -	— ,0020 grs.
But the sum of the errors of the six troy pounds is - - - - -	+ ,0039
Therefore the mean of the errors of the two 6 lb. weights is - - -	+ ,0019
The first comparisons of the five avoirdupois pounds with one of the 6 lb. weights + 440 grains gave for the difference - - - - -	— ,0630 grs.
With the other 6 lb. weight + 440 grains, the difference was - - -	+ ,0260
The mean is - -	— ,0185
But the mean error of the two 6 lb. weights is	+ ,0019
Therefore the sum of the errors of the five avoirdupois pounds is - - -	— ,0166

The sum of the differences between No. 1. and the other avoirdupois pounds being ,019 grains, and all in defect, wires of the following values were enclosed in each weight; viz.

In No. 2. - -	,002 grs.
3. - -	,002
4. - -	,006
5. - -	,007
Total -	,017

The very near agreement of the preceding result with Mr. BATE's determination, is a sufficient proof of his care, and of the accuracy with which the avoirdupois pound has been obtained.

As each of the avoirdupois pounds had certain wires enclosed in it when delivered to me by Mr BATE, the following is an account of the wires they now contain.

No. 1. contains 1 wire = ,053 grains.
2. — 4 wires = ,184
3. — 4 wires = ,200
4. — 3 wires = ,250
5. — not noted.

After the adjustment of the avoirdupois pounds was completed, Mr. BATE used the large balance to make an approximate adjustment of the weights of the imperial gallon of distilled water. This was interrupted by an accident which rendered it necessary to repair the knife edges of the beam; and notwithstanding every care, the balance afterwards proved less certain in its indications than it was before the accident. The error arising from this source I have endeavoured to lessen by increasing the number of comparisons.

Adjustment of the weights of the imperial gallon of water.

These weights were distinguished in the following manner:

That which I shall call

No. 1. W^t OF IMP. GALL. OF WATER AT 62° F

No. 2. W^t OF IMP. GALL. OF WATER AT 62° F. a dot after the letter F.

No. 3. W^t OF IMP. GALL. OF WATER AT 62° F a dot after the figures.

No. 4. W^t OF IMP. GALL. OF WATER AT 62° F. a dot after the figures, and another dot after the letter F.

The two 6 lb. troy weights with 880 grains, making together ten pounds avoirdupois, were employed as a standard in the manner which has been already detailed in describing the adjustment of the troy pounds.

From previous rough trials, certain wires were added to each of the gallon weights, the value of which will be given hereafter, and the following comparisons were made. The index of the balance varied one division with one-tenth of a grain.

The two 6 lb. weights + 880 grains, Divisions.	Gallon weights.				The two 6 lb. weights + 880 grains.
	No. 1. Divisions.	No. 2. Divisions.	No. 3. Divisions.	No. 4. Divisions.	
+ 2,5	− 3,2	+ 2,5	+ 0,8	+ 5,4	+ 6,8
+ 5,8	+ 3,9	+ 2,7	− 1,5	+ 2,1	+ 4,3
− 0,6	− 1,4	− 3,3	− 1,7	− 2,1	− 1,7
− 0,8	− 6,4	− 4,1	− 0,9	− 8,1	+ 0,4
− 0,1	− 2,8	− 0,2	+ 1,5	+ 1,2	− 2,5
− 3,0	− 2,0	− 3,3	− 4,9	− 4,1	− 2,2
+ 0,63	− 1,98	− 0,95	− 1,12	− 0,93	+ 0,85
+ 0,85					
+ 0,74	Mean.				

By the above comparisons, the error of each weight in parts of a grain is as follows:

No. 1.	No. 2.	No. 3.	No. 4.
—,272	—,169	—,186	—,167

The two 6 lb. weights + 880 grains, Divisions.	Gallon weights. No. 1. Divisions.	No. 2. Divisions.	No. 3. Divisions.	No. 4. Divisions.	The two 6 lb. weights + 880 grains Divisions.
— 1,9	— 1,9	— 2,0	— 0,6	— 1,9	+ 1,7
+ 3,0	— 1,0	— 4,9	— 3,3	+ 1,0	+ 3,4
+ 2,9	+ 0,8	+ 0,6	— 1,9	+ 3,3	+ 3,3
+ 4,2	+ 2,9	— 1,7	+ 0,5	+ 1,9	+ 2,8
+ 1,4	0,0	+ 0,2	— 1,8	+ 3,9	+ 4,1
+ 2,5	+ 1,2	+ 1,6	+ 0,5	+ 4,8	+ 0,5
+ 1,3	+ 2,0	+ 3,1	— 6,6	+ 4,7	+ 5,2
+ 3,0	+ 4,0	— 3,2	+ 0,3	— 1,0	+ 3,4
+ 2,05 + 3,05	+ 1,00	— 0,79	— 1,61	+ 2,09	+ 3,05
+ 2,55	Mean.				

By the above comparisons, the error of each weight in parts of a grain, is as follows:

No. 1.	No. 2.	No. 3.	No. 4.
—,155	—,334	—,416	—,046

The two 6 lb. weights + 880 grains, Divisions.	Gallon weights. No. 1. Divisions.	No. 2. Divisions.	No. 3. Divisions.	No. 4. Divisions.	The two 6 lb. weights + 880 grains, Divisions.
+ 4,0	+ 1,2	+ 3,1	+ 2,5	+ 6,2	+ 7,2
+ 3,5	+ 3,0	— 0,4	— 1,5	— 1,0	— 0,2
— 5,0	— 1,7	— 2,6	— 3,0	— 3,8	— 0,5
— 1,9	— 0,8	— 1,3	— 3,4	+ 0,1	+ 2,7
+ 2,9	— 4,9	— 1,6	— 0,4	+ 1,1	+ 1,6
+ 3,7	+ 2,2	+ 1,2	— 3,9	+ 1,3	+ 3,0
+ 1,20 + 2,30	— 0,17	— 0,27	— 1,61	+ 0,65	+ 2,30
+ 1,75	Mean.				

By the above comparisons the error of each weight in parts of a grain is as follows :

No. 1.	No. 2.	No. 3.	No. 4.
—,192	—,202	—,336	—,110

Results of the comparisons of the four weights of the imperial gallon of water.

	No. 1. Grains.	No. 2. Grains.	No. 3. Grains.	No. 4. Grains.
1st series . .	—,272	—,169	—,186	—,167
2nd series . .	—,155	—,334	—,416	—,046
3d series . .	—,192	—,202	—,336	—,110
Mean errors .	—,206	—,235	—,313	—,108

A wire equal to its error was now enclosed in each weight.

The numbers and values of the wires inclosed in the gallon weights are as follow :

		Grains.
No. 1. contains	2 wires, together	= 0,366
No. 2. —	4 wires, —	= 1,174
No. 3. —	3 wires, —	= 0,791
No. 4. —	3 wires, —	= 0,422

The adjustment of the gallon weights occupied me till the 10th of April, when the balance was removed to Mr. BATE's for the purpose of adjusting the standard gallon measures.

Adjustment of the imperial gallon measures.

It has been remarked that the form chosen for the gallon measure is that of a hollow cone, terminated by a cylindrical mouth of about an inch and a half diameter. The inside of

the cone is turned very smooth and close to its base is curved so as to avoid the acute angle, which would have resulted from continuing the side of the cone to the bottom. By this also the advantage was gained of a greater substance for the insertion of the screws used for attaching the bottom of the gallon to the cone.

As soldering was thought objectionable, the bottom was ground to the cone, and it was supposed that it would thus have been sufficiently secure; but on letting the gallons remain filled for 24 hours, in some a slight leakage became perceptible. After many experiments, the best remedy for this appeared to be a very minute quantity of fresh grease, carefully applied to the bottom of the cone and then almost wholly wiped off; after which the bottom was firmly screwed in its place.

The following was the method pursued in adjusting the gallon:

If the gallon contained a considerable quantity, as one or two hundred grains of water too little, its capacity was enlarged by turning away a small portion from the flat bottom. If on the contrary the gallon contained too much, the base of the cone was ground away; and it must be evident that a very small quantity taken from this part would occasion a considerable variation in the capacity, one thousandth of an inch making a difference of about 17 grains. Errors of smaller magnitude were removed either by grinding down the top of the mouth, or by enlarging the aperture by using a cylindrical plug of brass with emery.

The interior of the measure being carefully wiped dry, it was placed in the scale together with one of the gallon

weights and a circular piece of plate glass (the use of which will be presently described), and the whole was counterpoised.

The division at which the index of the balance stood was noted, and the gallon weight removed; the gallon was then nearly filled with distilled water by means of a small glass syphon, so contrived by Mr. BATE as to prevent the introduction of bubbles of air. The temperature of the water was then taken as well as the height of the barometer, and the filling of the gallon continued until the water rose perceptibly a little above its mouth. The piece of plate glass beforementioned, and which had a small hole drilled in its centre, was then carefully placed upon the top of the gallon, when the superabundant water passed through the hole to the upper surface of the glass, and was removed by drawing it with the mouth into a capillary glass tube.

The difference of the expansion of water, and of brass, being an object of the highest importance in the present operations, I was glad to find that Mr. BATE had made numerous experiments upon this subject, and I had had sufficient experience of his care to place great reliance upon his conclusions. It will be seen that the considerable range of temperature under which the experiments with the gallons No. 1, 3, and 4, were made, and the uniformity of the results fully justify this confidence, and form very conclusive evidence of the accuracy of the corrections employed.

But besides the correction for the difference of the expansion of water and of the brass vessel, another allowance is necessary for the buoyancy of the atmosphere in cases where extreme accuracy is required. The gallon is to contain ten

pounds of distilled water, at the temperature of 62° of FARENHEIT's thermometer, the barometer being at 30 inches; consequently, for any difference from this state of the barometer, as well as of the thermometer, a correction must be applied. Water is 831 times heavier than air when the barometer is at 30 inches, and the thermometer at 62°, and this varies directly as the height of the barometer, consequently the correction due to a difference of one inch of the barometer will be $\frac{1}{831} \times \frac{1}{30} = \frac{1}{24930}$ part of the weight of water.

The gallon weight being counterpoised with brass, and brass being about 8 times heavier than water, the effect of the buoyancy of the atmosphere upon brass will be only $\frac{1}{8}$ of that upon water, and this will tend to lessen the effect upon the water by one eighth part of the whole quantity. Therefore $\frac{1}{24930} \times \frac{7}{8} \times 70000$ grains $= 2{,}46$ grains, is the number of grains by which the weight of the gallon of water will be increased by a depression of one inch of the barometer. Though the variation of the temperature of the air during the experiments was several degrees, the effect upon the correction for the buoyancy of the atmosphere would have been so inconsiderable as to be unworthy of notice upon the present occasion.

As an error of one degree in the determination of the temperature of the water contained by the gallon would occasion an error ranging from four to six grains, it must be evident that a knowledge of the precise temperature of the water was of primary importance. For this purpose a thermometer was used which had been prepared with extraordinary care by Mr. BATE, and the scale was such that its indications were estimated without difficulty to tenths of a degree.

It would perhaps be useless to detail the numerous experiments which were made in approximating to the final adjustment of the gallon measures, and I shall therefore immediately proceed to state the ultimate results.

Gallon (which I shall designate) No. 1.

Date.	Barom.	Temp. of the Water.	Weight of water contained by the gallon. 10 lbs. ±	Correction for Barometer.	Correction for Temperat.	Weight of water the gallon should have contained. 10 lbs. ±	Error in grains.	
June.	Inches.	o	Grains.	Grains.	Grains.	Grains.		
1	30,29	59,0	+ 13,13	— 0,71	+ 12,38	+ 11,67	+ 1,46	Mr. Bate.
6	29,94	59,4	+ 11,14	+ 0,15	+ 10,83	+ 10,98	+ 0,16	Mr. Bate.
11	30,22	66,6	— 24,96	— 0,54	— 23,29	— 23,83	— 1,13	
	30,16	67,8	— 30,84	— 0,40	— 30,17	— 30,57	— 0,27	Mr. Bate.
15	30,27	67,2	— 27,21	— 0,66	— 26,68	— 27,34	+ 0,13	
						Mean	+ 0,07	

The bottom of the gallon No. 2. was rather thin, it was therefore subsequently adjusted by Mr. Bate, and sent to the Exchequer to be used on more ordinary occasions.

Gallon No. 3.

Date.	Barom.	Temp. of the Water.	Weight of water contained by the gallon. 10 lbs. ±	Correction for Barometer.	Correction for Temperat.	Weight of water the gallon should have contained. 10 lbs. ±	Error in grains.	
May.	Inches.	o	Grains.	Grains.	Grains.	Grains.		
18	30,33	57,9	+ 16,52	— 0,81	+ 16,34	+ 15,53	+ 0,99	
20	30,29	57,4	+ 18,48	— 0,71	+ 18,02	+ 17,31	+ 1,17	
23	29,96	60,5	+ 6,67	+ 0,10	+ 6,44	+ 6,54	+ 0,13	
31	30,30	59,0	+ 12,58	— 0,74	+ 12,38	+ 11,64	+ 0,94	Mr. Bate.
June	30,30	59,2	+ 9,78	— 0,74	+ 11,60	+ 10,86	— 1,08	Mr. Bate.
1	30,30	58,8	+ 12,72	— 0,74	+ 13,10	+ 12,36	+ 0,36	
11	30,18	67,3	— 31,53	— 0,45	— 27,26	— 27,71	— 3,82	Mr. Bate.
13	30,14	67,4	— 26,74	— 0,35	— 27,84	— 28,19	+ 1,45	
	30,14	68,2	— 31,80	— 0,35	— 32,54	— 32,89	+ 1,09	
						Mean	+ 0,14	

Gallon No. 4.

Date.	Barom.	Temp. of the Water.	Weight of water contained by the gallon. 10 lbs. ±	Correction for Barometer.	Correction for Temperat.	Weight of water the gallon should have contained. 10 lbs. ±	Error in grains.	
June.	Inches.	°	Grains.	Grains.	Grains.	Grains.		
6	29,94	59,2	+ 12,36	+ 0,15	+ 11,60	+ 11,75	+ 0,61	Mr. BATE.
8	29,95	61,7	+ 2,36	+ 0,12	+ 1,31	+ 1,43	+ 0,93	
11	30,22	66,9	— 19,69	— 0,54	— 24,96	— 25,50	+ 5,81	
	30,16	68,2	— 28,58	— 0,40	— 32,54	— 32,94	+ 4,36	Mr. BATE.
13	30,14	67,0	— 25,30	— 0,35	— 25,52	— 25,87	+ 0,57	
14	30,28	68,8	— 36,42	— 0,69	— 36,17	— 36,86	+ 0,44	Mr. BATE.
15	30,25	67,9	— 30,84	— 0,61	— 30,75	— 31,36	+ 0,52	
						Mean	+ 1,89	
						Mean, rejecting greatest and least	+ 0,61	

Gallon No. 5.

Date.	Barom.	Temp. of the Water.	Weight of water contained by the gallon. 10 lbs. ±	Correction for Barometer.	Correction for Temperat.	Weight of water the gallon should have contained. 10 lbs. ±	Error in grains.	
June.	Inches.	°	Grains.	Grains.	Grains.	Grains.		
1	30,30	58,7	+ 12,83	— 0,74	+ 13,47	+ 12,73	+ 0,10	
	30,27	60,25	+ 5,80	— 0,66	+ 7,47	+ 6,81	— 1,01	Mr. BATE.
3	29,70	60,6	+ 7,91	+ 0,74	+ 6,02	+ 6,76	+ 1,15	
8	29,95	61,4	+ 2,80	+ 0,12	+ 2,63	+ 2,75	+ 0,05	
						Mean	+ 0,07	

Adjustment of the Quart and Pint for the Exchequer.

The quart and pint measures for the Exchequer differed in no respect from the gallon, except in being of inferior dimensions, and were adjusted in a similar manner.

Quart.

Date.	Barom.	Temp. of the Water.	Weight of water contained by the quart. 2½ lbs. ±	Correction for Barometer.	Correction for Temperat.	Weight of water the quart should have contained. 2½ lbs. ±	Error in grains.
June.	Inches.	°	Grains.	Grains.	Grains.	Grains.	
14	30,26	69,0	— 9,82	— 0,16	— 9,34	— 9,50	— 0,32
15	30,27	67,1	— 6,98	— 0,16	— 6,52	— 6,68	— 0,30
	30,27	67,5	— 6,86	— 0,16	— 7,11	— 7,27	+ 0,41
	30,27	68,1	— 8,12	— 0,16	— 7,98	— 8,14	+ 0,02
						Mean	— 0,05

Pint.

Date.	Barom.	Temp. of the Water.	Weight of water contained by the pint. 1¼ lbs. ±	Correction for Barometer.	Correction for Temperat.	Weight of water the pint should have contained. 1¼ lbs. ±	Error in grains.
June.	Inches.	°	Grains.	Grains.	Grains.	Grains.	
15	30,27	67,6	— 3,63	— 0,08	— 3,55	— 3,63	0,00
18	30,18	63,0	— 0,53	— 0,05	— 0,58	— 0,63	+ 0,10
	30,18	63,4	— 0,70	— 0,05	— 0,82	— 0,87	+ 0,17
						Mean	+ 0,09

Verification of the Bushel measures.

The weight of the bushel measure, together with the 80 lbs. of water it should contain, was about 250 lbs. and as I could find no balance capable of determining so large a weight with sufficient accuracy, I was under the necessity of constructing one for this express purpose.

I first tried cast iron; but though the beam was made as light as was consistent with the requisite degree of strength, the inertia of such a mass appeared to be so considerable, that much time must have been lost before the balance would have answered to the small differences I wished to ascertain. Lightness was a property essentially necessary, and bulk was very desirable in order to preclude such errors as might arise from the beam being partially affected by sudden alterations of temperature. I therefore determined to employ wood, a material in which the requisites I sought were combined. The beam was made of a plank of mahogany about 70 inches long, 22 inches wide, and $2\frac{1}{4}$ thick, tapering from the middle to the extremities. An opening was cut in the centre, and strong blocks screwed to each side of the plank to form a bearing for the back of a knife edge which passed through the centre. Blocks were also screwed to each side at the extremities of the beam on which rested the backs of the knife edges for supporting the pans. The opening in the centre was made sufficiently large to admit the support hereafter to be described, upon which the knife edge rested.

In all beams which I have seen, with the exception of those made by Mr. ROBINSON, the whole weight is sustained by short portions at the extremities of the knife edge, and the weight being thus thrown upon a few points, the knife edge becomes more liable to change its figure and to suffer injury.

To remedy this defect, the central knife edge of the beam I am describing was made six inches, and the two others five inches long. They were triangular prisms with equal sides, of three quarters of an inch, very carefully finished, and the edges ultimately formed to an angle of 120°.

Each knife edge was screwed to a thick plate of brass, the surfaces in contact having been previously ground together, and these plates were screwed to the beam, the knife edges being placed in the same plane, and as nearly equidistant and parallel to each other as could be done by construction.

The support upon which the central knife edge rested throughout its whole length was formed of a plate of polished hard steel screwed to a block of cast iron. This block was passed through the opening before-mentioned in the centre of the beam and properly attached to a frame of cast iron.

The stirrups to which the scales were hooked rested upon plates of polished steel to which they were attached, and the under surfaces of which were formed by careful grinding into cylindrical segments. These were in contact with the knife edges their whole length, and were known to be in their proper position by the correspondence of their extremities with those of the knife edges.

A well-imagined contrivance was applied by Mr. BATE for raising the beam when loaded, in order to prevent unnecessary wear of the knife edge; and for the purpose of adjusting the place of the centre of gravity, when the beam was loaded with the weight required to be determined, a screw carrying a moveable ball projected vertically from the middle of the beam.

The performance of this balance fully equalled my expectations. With two hundred and fifty pounds in each scale, the addition of a single grain occasioned an immediate variation in the index of one-twentieth of an inch, the radius being fifty inches.

In using this beam, care should be taken that the ends of

the steel plates to which the stirrups are attached coincide with the ends of the knife edges, otherwise some error might arise from a possible want of parallelism.

The bushel measure being placed in one of the scales, together with 80 lbs. * the whole was counterpoised with brass; the 80 lbs. was then removed, and the bushel filled with water by means of a syphon. The temperature of the water was then very carefully taken, and the height of the barometer registered. A circular piece of plate glass, as plane as could be procured, having a hole about a quarter of an inch diameter in its centre, was slid over the bushel, and the air-bubbles which appeared on the under surface of the glass were removed by touching them with a small bent tube made of flexible metal, a method thought of by Mr. BATE. Water was added through the hole in the glass; and by carefully observing the curvature of the fluid surface, it was soon evident that the filling of the bushel might be repeated without a greater difference than three or four grains.

It is scarcely requisite to remark that many experiments were made, not here detailed, before the adjustment of the bushel was perfected; the very great accuracy and masterly workmanship however of Mr. KEIR, who had been furnished with gauges for the diameter and depth of the bushel, rendered little alteration necessary.

* This was obtained by taking twice the weight of the four weights of the gallon of water.

Bushel No. 1.

Date.	Barom.	Temp. of the Water.	Weight of water contained by the bushel. 80 lbs. ±	Correction for Barometer.	Correction for Temperat.	Weight of water the bushel should have contained. 80 lbs. ±	Error in grains.	
June.	Inches.	°	Grains.	Grains.	Grains.	Grains.		
22	30,00	60,30	+ 61,18	0, 0	+ 58,13	+ 58,13	+ 2,97	Mr. Bate,
24	29,98	61, 4	+ 34,46	+ 0,40	+ 21,01	+ 21,41	+ 12,99	
	29,96	61,75	+ 12,55	+ 0,10	+ 8,75	+ 8,85	+ 3,70	
						Mean	+ 6,55	

Bushel No. 2.

Date.	Barom.	Temp. of the Water.	Weight of water contained by the bushel. 80 lbs. ±	Correction for Barometer.	Correction for Temperat.	Weight of water the bushel should have contained. 80 lbs. ±	Error in grains.	
July.	Inches.	°	Grains.	Grains.	Grains.	Grains.		
7	30,00	63,1	— 38,00	0,00	— 41,00	— 41,00	+ 3,00	
8	29,98	62,5	— 18,10	+ 0,40	— 18,49	— 18,09	— 0,01	
	29,98	62,6	— 19,59	+ 0,40	— 22,19	— 21,79	+ 2,20	
						Mean	+ 1,73	

Bushel No. 3.

Date.	Barom.	Temp. of the Water.	Weight of water contained by the bushel. 80 lbs. ±	Correction for Barometer.	Correction for Temperat.	Weight of water the bushel should have contained. 80 lbs. ±	Error in grains.	
May.	Inches.	°	Grains.	Grains.	Grains.	Grains.		
18	30,33	57,4	+141,65	— 6,48	+144,16	+137,68	+ 3,97	
20	30,29	57,7	+140,54	— 5,68	+136,06	+130,38	+10,16	
24	29,83	62,3	+ 1,54	+ 3,36	— 11,09	— 7,73	+ 9,27	Mr. Bate.
31	30,28	59,3	+ 96,20	— 5,52	+ 89,73	+ 84,21	+11,99	Mr. Bate.
June.								
17	30,16	66,8	—201,50	— 3,20	—195,25	—198,45	— 3,05	Mr. Bate.
						Mean	+ 6,47	

Bushel No. 4.

Date.	Barom.	Temp. of the Water.	Weight of water contained by the bushel, 80 lbs. ±	Correction for Barometer.	Correction for Temperat.	Weight of water the bushel should have contained, 80 lbs. ±	Error in grains.	
June.	Inches.	°	Grains.	Grains.	Grains.	Grains.		
27	29,77	61,9	+ 6,10	+4,48	+ 3,50	+ 7,98	— 1,88	
	29,77	62,0	+ 5,60	+ 4,48	0,0	+ 4,48	+ 1,12	
	29,79	61,9	+ 5,20	+ 4,16	+ 3,50	+ 7,66	— 2,46	
						Mean	— 1,07	

In order to enable the reader to form some idea of the errors of the measures of capacity, it may be remarked that a drop of water is commonly estimated to be nearly equal to one grain.

As the quantity of water contained by the bushel measure is determined by the surface of the glass plate, which should be a perfect plane, a figure not easily attainable, it is not to be understood that the capacity of the bushel is true within the limits of the errors above-mentioned, but to the gallon, quart, and pint measures, in consequence of their more advantageous form, this source of error does not in any sensible degree apply.

Verification of the standard yards with steel terminations.

These standard yards were made by Mr. DOLLOND; they are of brass, one inch square. To their extremities are firmly screwed rectangular pieces of steel of the same width as the bar, and projecting above its surface. The distance between the interior faces of the steel termination is intended to be equal to the length of the imperial standard yard.

To determine this distance I employed the following method:

Two bars of brass were prepared, three quarters of an inch square and rather less than 18 inches long. They were terminated by planes at right angles to their length; and upon the upper face of each bar, very near to the end, a fine transverse line was drawn; the other ends of the bars being then placed in contact and kept so by springs, the distance between the lines was taken by means of two micrometer microscopes fixed to a bar of wood and referred to Sir G. SHUCKBURGH'S standard scale, which scale, it has already been remarked, does not sensibly differ from the imperial standard yard.

The distance between the lines was found by numerous comparisons to be 919 divisions of the micrometer less than the standard yard, each division of the micrometer being equal to $\frac{1}{23363}$ of an inch.

The brass bars were then placed upon the standard to be examined, their marked ends being next each other, and their opposite extremities kept in contact with the steel faces by a spring introduced between the bars, a part below the surface being cut away for that purpose. The distance between the lines was then measured by the micrometer microscope, which distance, had the standard been perfectly correct, would have been equal to what the distance of the lines in the former position of the bars wanted of 36 inches.

Standard Yard, No. 1.

The distance between the lines upon the brass bars was found by the mean of six measurements to be 918,2 divisions of the micrometer, which differs so little from 919 divisions,

that this standard yard may be considered as perfectly correct.

Standard Yard, No. 2.

The mean of sixteen measurements gave the distance between the lines upon the brass bars 910 divisions. This standard is therefore 9 divisions, or ,00098 of an inch too short.

Standard Yard, No. 3.

By the mean of six measurements this standard appeared to be five divisions, or ,00021 of an inch too long.

Standard Yard, No. 4.

The mean of ten measurements gave the error of this standard five divisions, or ,00021 of an inch too short.

Adjustment of the Standard Yards with gold points.

The standard yards last described are intended merely for the purpose of sizing those employed in commerce, and the trifling differences above stated may be utterly disregarded; but the Commissioners of Weights and Measures thought it desirable that accurate copies of the imperial standard yard should be made, to be carefully preserved and transmitted to posterity solely for the purpose of being referred to upon extraordinary occasions, or upon questions important to science.

The difficulty of transferring a given distance from one scale to another, is well known to all who are acquainted with the subject; the operation is one of considerable delicacy; and notwithstanding every precaution, is seldom abso-

lutely free from error. But a national standard should be accurately that which it professes to be. It is not enough to determine its error, as the record of this may in process of time be lost; it therefore became necessary to devise a method by which any perceptible error in those standards which are the foundation of all the others, might ultimately be annihilated.

The four standard yards which I am about to describe are of brass, one inch and a quarter wide, and half an inch thick. This thickness is the same as that of Sir G. SHUCKBURGH'S scale, and was chosen in order that both might be affected with equal readiness by any change of temperature; for as the imperial standard yard of 1760 is one inch square, I thought it preferable to adjust the new standards by means of Sir G. SHUCKBURGH'S scale, which, as I have before remarked, does not sensibly differ from it.

A disk of gold being let into the surface near one extremity, a hole was drilled through the bar at the distance of thirty-six inches from the centre of the disk, and being made slightly conical, a plug of brass was ground in the hole so as to fit it perfectly. A gold disk was let into the top of the plug, and reduced to a level with the surface of the scale. The other end of the plug projected beneath the scale, and had a small hole through it to admit a wire, by means of which it might be turned round. A very fine deep dot was then made by Mr. DOLLOND upon each of the gold disks, as nearly as it could be done at the distance of thirty-six inches from each other, the dot upon the moveable disk not being exactly in its centre.

Before the plug was ground in its place a small hole was drilled through the side of the scale into the conical aperture.

The microscopical apparatus employed on the present occasion has been described in the paper upon the comparison of various British standards of linear measure before quoted.

The cross wires of the microscopes being brought respectively over zero, and 36 inches upon Sir G. SHUCKBURGH'S scale, the apparatus was transferred to the new standard, and the intersection of the cross wires of one of the microscopes placed upon the centre of the fixed dot. The moveable dot was then brought by turning the brass plug to the intersection of the cross wires of the other microscope.

The distance of the dots was repeatedly compared with Sir G. SHUCKBURGH'S standard upon different days, in order to ascertain that no perceptible error remained. A drill was passed through the hole in the side of the scale, and the brass plug carefully pierced through; a pin was then driven into the plug so as to render any change of position impossible, and the projecting part of the plug was cut off.

The standards being thus finished, they were again compared with Sir G. SHUCKBURGH'S scale, and it was with surprise and disappointment that I found the whole of them apparently too short. They had been adjusted upon a board of mahogany carefully planed, and the table upon which they were now placed was so flat as to occasion little alteration in a spirit level passed along it. The error of the standards was however far too considerable to be attributed to any curvature which on this occasion could take place, and it was

not until after several days that I discovered the cause of this perplexing circumstance. I found that by placing a card, the thickness of which was accurately one-fiftieth of an inch, under the middle of the standard, the distance of the dots was much increased, and by placing a card of the same thickness under each of the extremities, and withdrawing that which was under the centre, the distance of the dots was considerably diminished. The total difference amounted to no less than ,0016 of an inch, whilst the double of the error which would have arisen from mere curvature under similar circumstances would not have been one ten-thousandth of an inch.

The cause was now evident; by elevating the middle of the standard, the under surface was shortened, and the upper surface extended; and on the contrary, when the extremities were elevated the upper surface was compressed and the lower surface lengthened; the quantity of the effect evidently depending upon the thickness of the bar.

Having thus assured myself of the source of the error, a method of obviating it soon presented itself. As the upper and under surfaces of the bar are in different states, the one being compressed and the other extended, there must be an intermediate plane which suffers neither extension nor compression, and this plane must be nearly midway between the two surfaces. I therefore caused Mr. DOLLOND to reduce the thickness of the bar for the distance of an inch and three quarters from its extremities to one half; the gold disks and plugs were then inserted as before, and the adjustment completed in the manner which has been described. The plugs being secured, and the projecting parts removed, the

standards were repeatedly compared with Sir G. SHUCKBURGH's scale (the standard being placed upon the scale) when no perceptible difference could be detected. Pieces of card were now placed under the standard as before, without occasioning any appreciable alteration; and I had thus experimental proof of the perfect efficiency of the remedy I had employed.

I have been thus particular in detailing the difficulties I experienced, because they exhibit a source of very considerable error which may arise from the thickness of a standard scale, and which I believe has never before been suspected.

It may be here not unnecessary to remark, that on every occasion on which I have used Sir G. SHUCKBURGH's scale, it has fortunately been placed not only upon the same table, but upon the same part of it.

The various standards which have been described in this paper with the exception of the yards with steel terminations are not meant for common use, but are intended to be carefully preserved, to be referred to only upon extraordinary occasions. In addition however to these, other weights as well as measures of capacity were made with great care by Mr. BATE. The following is a list of the whole, with an account of the places where they are deposited.

Standards deposited at the Exchequer, Westminster.

1 Imperial standard yard with gold points.
1 Standard yard with steel terminations, No. 1.
1 Imperial troy pound, No. 5.
1 Avoirdupois pound, No. 1.

1 Avoirdupois pound, No. 5. (in a box with smaller weights.)
1 Weight of imperial gallon of water, No. 1.
1 Imperial gallon measure, No. 3.
1 Bushel, No. 3.
1 Quart, No. 4.
1 Pint.

A copy of the imperial Gallon,
Quart, and
Pint.

1 Bushel,
1 Half bushel,
1 Peck,
} for common use.

1 Gallon,
1 Half gallon,
1 Quart,
1 Pint,
1 Half pint,
1 Gill,
1 Half gill,
} cylindrical, for common use.

1 Set of avoirdupois weights, from 56 lbs. to half a drachm.
1 Set of counterpoises for the above set of weights.
1 Set of troy weights, from one pound to one grain, with counterpoises.

Standards deposited at Guildhall, London.

1 Imperial standard yard with gold points.
1 Standard yard with steel terminations, No. 4.
1 Imperial troy pound, No. 1.
1 Avoirdupois pound, No. 2.

1 Weight of imperial gallon of water, No. 3.
1 Imperial gallon measure, No. 5.
1 Bushel, No. 4.
1 Quart.
1 Pint.
1 Set of avoirdupois weights, from 56 lbs. to half a drachm.

Standards deposited at Edinburgh.

1 Imperial standard yard with gold points.
1 Standard yard with steel terminations, No. 2.
1 Imperial troy pound, No. 2.
1 Avoirdupois pound, No. 3.
1 Weight of imperial gallon of water, No. 4.
1 Imperial gallon measure, No. 4.
1 Bushel, No. 1.
1 Quart.
1 Pint.
1 Set of avoirdupois weights, from 56 lbs. to half a drachm.

Standards deposited at Dublin.

1 Imperial standard yard with gold points.
1 Standard yard with steel terminations, No. 3.
1 Imperial troy pound, No. 4.
1 Avoirdupois pound, No. 4.
1 Weight of imperial gallon of water, No. 2.
1 Imperial gallon measure, No. 1.
1 Bushel, No. 2.
1 Quart.
1 Pint.
1 Set of avoirdupois weights, from 56 lbs. to half a drachm.

I cannot conclude without bearing testimony to the unwearied perseverance, ability and accuracy, which Mr. BATE has shown in the course of a work attended with no common difficulties, and to the perfect execution of which he has devoted, for a long period, the whole of his time and attention.

From what has been said, it will be seen that the length of the pendulum vibrating seconds in London has been found in parts of the imperial standard yard; consequently, the value of the yard may at any time be known, having been referred to a natural standard presumed to be unalterable. The length of the French mètre, a standard expressing a certain portion of a terrestrial meridian, has also been given in parts of the English scale. The weight of a cubic inch of distilled water has been determined in parts of the imperial troy pound; and thus the pound, if lost, may at any future age be recovered. The avoirdupois pound is now for the first time defined, and the measures of capacity are made to depend upon the weight of water they contain; the imperial gallon, containing ten pounds avoirdupois of water, having been declared to be the unit or only standard measure of capacity from which all others are to be derived. This it is to be presumed cannot but powerfully tend to produce uniformity throughout the United Kingdom, by putting it in the power of every individual possessed of standard weights, to verify his measures of capacity with the utmost facility.

London, November, 1825.

APPENDIX.

Table of the correction on account of temperature to be applied to the contents of the Gallon.

Temperat. Fahrenheit.	Grains.	Difference for 1°
°		
50	+ 35,454	1,481
51	+ 33,972	1,758
52	+ 32,214	2,033
53	+ 30,181	2,304
54	+ 27,877	2,573
55	+ 25,304	2,839
56	+ 22,464	3,102
57	+ 19,362	3,363
58	+ 15,999	3,620
59	+ 12,379	3,875
60	+ 8,504	4,127
61	+ 4,376	4,376
62	0,000	4,623
63	— 4,623	4,866
64	— 9,489	5,107
65	— 14,596	5,345
66	— 19,942	5,580
67	— 25,522	5,813
68	— 31,335	6,042
69	— 37,377	6,269
70	— 43,646	

POSTSCRIPT.

Since the preceding Paper was read, I have received a work on Astronomy, published at Tübingen in 1811, by Professor BOHNENBERGER. This work had been pointed out to me by Professor SCHUMACHER as containing a description of the convertible pendulum, and a suggestion of its employment for the purpose of determining the distance between the centres of suspension and oscillation.

A friend has favoured me with a translation of the part in question, by which I find M. SCHUMACHER's information to be perfectly correct, and that the conception of the convertible pendulum is not so new as I had imagined when I first engaged in this enquiry. After demonstrating the reciprocity of the centres of suspension and oscillation, the author proceeds to say: "On a cylindrical or prismatic rod C A "(fig. 102.) let there be placed two wedge-formed axes "at C and *c*, whose edges being turned towards each other, "are perpendicular to the rod and parallel to each other. "Let the one be at the end C of the rod, and the other at *c*, "distant from C something more than two-thirds the length "of the rod, so that the centre of oscillation *o*, about the "edge C, may fall between C and *c*. On the remaining "part, *c* A of the rod, let a small weight *n*, slide backwards "and forwards. Now, by a diminution of the mass of the "rod on one or on the other side, it is easy to manage, that "if this pendulum be suspended on its edge C, a plumb line "hanging down from this, falls upon the edge *c*, conse-"quently the centre of gravity of the pendulum falls in the "plane of the axis of rotation. By sliding the weight *n*, the

" centre of oscillation round C may be made to fall in *c*, " which is known by the oscillations on C and *c* being " isochronous. Therefore the distance of the edges is equal " to the length of the simple pendulum which is isochronous " with this compound pendulum."

Although it does not appear that this idea was ever put in practice, it is evident, from the above extract, that the first proposal to determine the length of the seconds pendulum by means of the convertible pendulum, belongs to Professor BOHNENBERGER, and I take the earliest opportunity of acknowledging his claims, in order that the credit of the first suggestion may rest where it is so justly due.

London, 31st December, 1825.

II. *Description of an improved Hygrometer.* *By Mr.* Thomas Jones. *Communicated by Captain* Henry Kater, *F.R.S.*

Read June 16, 1825.

The attention of the scientific world has been lately so much occupied in experiments on atmospheric phenomena, that it is hoped any simplification or improvement in the instruments employed for that purpose, may not be unacceptable.

The principle of the hygrometer which I am about to describe, is that of enabling the observer, readily and accurately, to ascertain by direct and simple means, the degree of temperature at which the moisture of the atmosphere is condensed, and the instant at which that operation commences.

The hygrometer is composed of a mercurial thermometer, the graduated scale of which is about four inches and a half long; at the lower part of the scale the glass tube is bent to form a right angle, at the end of which the bulb of the thermometer rises parallel to the scale, and about one inch from it; the bulb is about one inch long, and of a cylindrical form, with a black convex top, the diameter of which is a little more than that of the cylindrical part, which is covered with silk. The scale is attached to a piece of cylindrical wire, three inches long, and turns upon a joint screw passing into its edge, the other end of which wire being placed in a tubular foot fixed to the inside of one end of the case, forms

a stand for the instrument. The case contains a small bottle for ether.

The thermometer thus constructed, will give both the temperature of the air and that of the dew point; which last is effected by placing the mouth of the bottle containing the ether, in contact with the upper part of the covered surface of the bulb, when by gently inclining the bottle, the ether will flow downwards without wetting the top of the bulb, which will almost immediately become dull by the deposition of moisture on its surface; when the observed temperature may be taken and the difference ascertained.

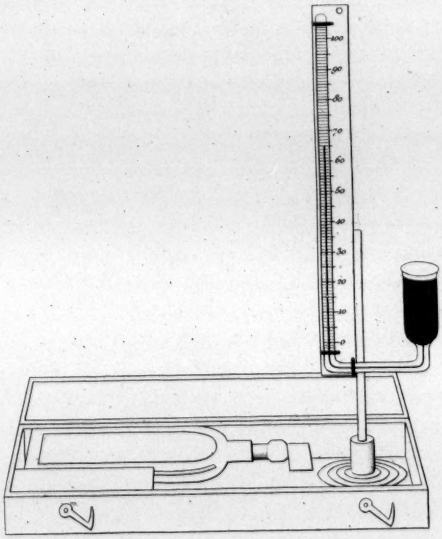
Should it be objected against the principle of the instrument here proposed, that the indications do not exhibit the true temperature of the upper surface of the bulb on which the deposition of dew takes place, but that of the lower part to which the ether is applied; it may be answered, that by inclining the whole instrument so as to render the axis of the bulb horizontal, and establish thereby a free circulation of the mercury in every part, this objection may be obviated; but on repeated trials I have not found this to produce any difference in the results.

I ought also perhaps to mention that an instrument somewhat similar in principle has been used in Vienna, and was mentioned by Professor BAUMGARTEN of that capital to a friend, who communicated the fact to myself.

The instrument is represented in Plate I. Fig. 1.

THOMAS JONES.

20th February, 1825.


O
100
90
80
70
60
50
40
30
20
10
0

III. *Observations on the changes which have taken place in some antient alloys of copper.* By JOHN DAVY, *M.D. F.R.S. In a Letter to Sir* HUMPHRY DAVY, *Bart. Pres. R. S.*

Read November 17, 1825.

PERMIT me through you to lay before the Royal Society, the results of some experiments and observations on the incrustations of certain antient alloys of copper, which I trust may not be undeserving of notice, whether considered in connexion with the arts of Antient Greece, or in relation to the slow play of chemical affinities acting during a long period of time.

I shall commence with the most interesting object that I have examined since I have been in the Mediterranean, for which I am indebted to His Excellency Sir FREDERICK ADAM, His Majesty's Lord High Commissioner in the Ionian islands. This is a bronze helmet of the antique Grecian form, recently found in a shallow part of the sea, between the citadel of Corfu, and the village of Castrades. Both internally and externally it is partially encrusted with shells, and a deposit of carbonate of lime. The surface of the helmet generally, both under the incrustation, and where freed from it, is of a variegated colour, mottled with spots of green, dirty white, and red. On minute inspection, the green and red patches exhibit a crystalline structure, and the red very distinctly; and on examining them with a lens, they were found to be

formed of octohedrons of the red oxide of copper, intermixed with crystals of the same form of metallic copper. These crystals are most distinct at the surface; they even adhere to the calcareous crust, and may be detached with it, presenting well-formed facets towards the part from which they have been separated. Chemical examination shows that they are actually composed of metallic copper, without the least tarnish, and of the pure ruby protoxide of copper; and that the green rust consists principally of carbonate and submuriate of copper, and the dirty white chiefly of oxide of tin. The mineralizing process (if I may be allowed the expression) which has produced these new combinations, in general has penetrated very little into the substance of the helmet. The incrustation and rust removed, the metal is found bright beneath; in some places considerably corroded, in others very slightly. It proves on analysis to be copper alloyed with 18.5 per cent. of tin. Its colour is that of our common brass, and it possesses a considerable degree of flexibility.

An antient nail from a tomb in Ithaca, and an antient mirror from a tomb at Samos, in Cephalonia, have afforded me results very similar to the preceding, excepting that the mineralized part, consisting of oxide of tin, submuriate, carbonate, and protoxide of copper, each more or less collected apart from the others into little masses, has exhibited rather a crystalline structure than distinct well-formed crystals, and has been without any appearance of crystals of metallic copper. The copper in the mirror I have found alloyed with about 6 per cent. of tin, and a very minute quantity of arsenic and zinc.

I have examined a select number of antient coins, purposely chosen from the fine collection of Dr. DEMETRIO PETRIZZOPULO, of Santa Maura, to whose kindness I am indebted for them. The results too which they have afforded are in conformity with the preceding. As this part of my subject may be interesting to the antiquarian, as well as to the chemical enquirer, I shall treat it a little in detail. The very pale incrustations, consisting of a mixture of green and white particles, contain a large proportion of oxide of tin, and a little carbonate of copper, or submuriate of copper, or both. They appear to owe their light hue to the accumulation of the oxide of tin at the surface. The black incrustations owe their colour to the predominance of the black oxide of copper, which is commonly mixed with a little protoxide of the same metal, and a minute quantity of oxide of tin, and of the carbonate and submuriate of copper. The coins that I have examined with this crust have contained very little tin. The bluish-green incrustations consist chiefly of carbonate of copper, either without oxide of tin, or mixed with a small proportion of this metal. The emerald-green incrustations abound in the submuriate of copper, and the red consist almost entirely of the protoxide of copper. These two compounds I have never witnessed spread over the whole of a coin, but more or less mixed with rusts of a different kind, studding the surface in the form of little crystalline elevations.

It is natural to enquire—is there any connexion between the chemical composition of the incrustations of antient coins, and their texture; some being remarkably smooth, and as

it were polished, preserving admirably the original impression of the die; whilst others are rough and deformed? I am disposed to reply in the negative, not having yet discovered such a connexion; excepting thus far, that the protoxide of copper, and its submuriate, frequently appear in minute crystalline elevations rising above the surface of the coin. Generally, I suspect the difference of texture in question depends on other circumstances, and very likely in different degrees of rapidity of operation of the mineralizing cause.

It is a curious question for consideration, the manner in which the crystalline structure is acquired exhibited by these incrustations; and still more so, how crystals are formed, especially such as were discovered on the helmet, and on the adhering calcareous deposit. There being no reason to suppose deposition from solution, are we not under the necessity of inferring, that the mineralizing process witnessed in its effects, depends on a slow motion and separation of the particles of the original compound? and, must we not conclude, that this motion is connected with the operation of electrochemical powers?* Supposing the oxides first formed upon the bronze in a state of negative electricity, with respect to the metal, their agency, in a long course of ages would be to separate the least oxidable metal, the copper, in its pure form; and all the changes are such as may be accounted for from agencies of the same kind, but of different intensity. And may not similar explanations be given of other like

* The separation of the copper from the tin in the bronzes found in the ancient cities destroyed by volcanic eruptions, must be referred to another source; namely, the crystallizations dependent upon degrees of heat a little below the point of fusion of the alloy.

phænomena, which often seem to occur in the mineral kingdom? These facts offer an obvious practical application, both in determining the antiquity and genuineness of ancient works of art; for ærugo of the kind which I have described could not be successfully imitated by any artificial processes; and a very slight chemical examination, or even one by the lens, would be sufficient to expose any fraud attempted to be practised by substituting modern imitations for ancient works.

I am, most truly yours,

J. DAVY.

To Sir H. Davy, Bart.
Pres. R. S.

IV. *Additional proofs of animal heat being influenced by the nerves.* By Sir Everard Home, *Bart. V. P. R. S.*

Read November 16, 1825.

The conclusions drawn from my experiment upon the deer's horns, published in the last volume of the Philosophical Transactions, are so important, that I felt it a duty to repeat the experiment in the ensuing season.

This I have done, and have the satisfaction to find that the result agrees in every respect with that drawn from the first experiment.

For the present experiment, I was furnished with a buck of full head, seven years old, and in good condition; the former not having suffered from what was done in any respect whatever.

As several disadvantages had arisen in the former experiment from the horns having been bored; they were now prevented by applying a cincture of knitted worsted round that part of the palm, the heat of which was to be ascertained, three inches broad; and the bulb of the thermometer was received between this and the velvet of the horn. As soon as the temperature was taken, the cincture was removed. All liability to external injury was precluded, by confining the animal in a stable.

A circumstance favourable to put the result of this experiment to the severest test was, the heat of the atmosphere being very different from that in the former season; during

the time of the experiment in 1824, it was 66°, in 1825, 84°, a difference of 18°.

This experiment was begun on the 12th of July, 1825, at three o'clock. The temperature of the atmosphere was 84°, that of both horns exactly 98½°. Immediately after Mr. MAYO had divided the nerves, the heat of that horn was diminished to 93¼°, and the heat of the other encreased to 99°.

The following register, showing the changes that took place in the temperature of the horns and atmosphere, was regularly filled up every day at three o'clock.

Day of July.	Heat of Atm.	Heat of Horn operated on.	Heat of the other Horn.
12th	84°	93¼°	99°
13th	84	93	98
14th	84	93	100
15th	85	96	99
16th	86	97	99¾
17th	86	97	99
18th	86	90¾	98
19th	86	97	99
20th	85	97	99
21st	75	95	97
22d	76	93	93
23d	74	93	93

After the two horns had become of the same temperature the buck was allowed to join the herd. On the 28th of July it was killed for the use of the table, which gave me an opportunity after death of examining the parts that had been operated on. The horns had not shed their velvet, were exactly of the same size, and equal in solidity.

The parts where the nerves had been divided were carefully dissected and examined by Mr. MAYO, Mr. CÆSAR HAWKINS, and myself. The two cut ends of the nerves had receded from one another, and the interval between them was occupied by a dense newly-formed substance, which had become a firm connecting medium, explaining satisfactorily the restoration that had taken place in their functions. In the former experiment this connecting medium was less dense, and left a doubt upon my mind, which is now completely removed.

While engaged in this investigation I have had an opportunity, in the course of my professional duty as Surgeon to Chelsea Hospital, to ascertain the effect of tying the trunk of the artery that supplies the human thigh and leg on one side, upon the temperature of the limb, compared with that of the other, which remained in a natural state; and contrasting this with the effect that has been stated to take place when the nerve of one horn is divided.

As I intend to lay before the Society the detail of the treatment of this case, of an aneurism in the external iliac artery of the right side, which I trust will tend to the advancement of the science of Surgery on a future occasion, I shall confine myself at present to the effect produced upon the temperature of the limb, when the femoral artery is obliterated immediately below Popart's ligament.

Before the artery was included in a ligature, the temperature of the foot, ascertained by a thermometer, was 95°; that of the left, which was in a state of perfect health, 84°. In ten minutes after the operation the heat of the foot fell to 87°; in 30 minutes to 85, while the left foot remained at 84°.

In eight hours the heat of the left foot rose to 94°, probably from the increased quantity of blood circulating through its arterial branches, since next day the temperature fell to 88°, at which it continued stationary for four days; the foot of the aneurismal side in 24 hours fell to 84°, but on the following day rose to 85°, and continued at or above that temperature till the ligature came away.

Nothing can have been more satisfactorily proved than that the heat of the parts of the body is diminished by dividing the nerves going to them; while, on the other hand, obstructing the arterial trunk of the thigh, even before it sends off the great muscular branch, does not diminish the heat in the smallest degree below the natural standard.

I have had many opportunities of ascertaining the temperature of the foot after the femoral artery has been tied in the operation for the popliteal aneurism, and found it commonly one degree below that of the other foot, but the heat of the two feet had never been ascertained before the operation, so that I was most probably deceived in the conclusion I had drawn; and in reality, the heat of the other foot had been raised above the standard, instead of having been depressed below it.

V. *The Croonian Lecture. On the structure of a muscular fibre from which is derived its elongation and contraction. By Sir* EVERARD HOME, *Bart. V. P. R. S.*

Read December 15, 1825.

IN the course of the last 40 years I have given this Lecture more frequently than any Member of the Society; and, like my predecessors, on those occasions, have taken up the subjects most nearly connected with the inquiry for which the Lecture was instituted.

As far back as the year 1818, while considering the mode in which coagulated blood is rendered vascular, I brought forward a magnified drawing of a muscular fibre made by Mr. BAUER, showing it to be composed of a single row of globules $\frac{1}{2,000}$ parts of an inch in diameter, or in other words, of red globules deprived of their colouring matter.

From that time I had not proceeded further in investigating muscular structure, but the appearance of the nervous fibres of the great splanchnic ganglion in Mr. BAUER's magnified drawings, which I laid before the Society last spring, led me to consider that the organization of these fibres must be so closely allied to that of muscles, that every physiologist who examined the drawings, must immediately come to the same conclusion; and no sooner would those drawings be in the hands of the public, than any one might with the greatest

ease complete the discovery by an actual examination of muscular fibres in the microscope.

Under this impression, I requested the President to appoint me to give this Lecture, which is to be read nearly at the same time my paper on ganglions will be published; so that no one will have an earlier opportunity of applying what is said of nerves in that communication to muscular fibres, the consideration of which is the object of the present Lecture.

In Mr. BAUER's former examination of muscular structure, that the integrant fibre might be more easily separated from the fasciculus to which it belonged, we had gone into the same error with those physiologists who have made diagrams of the internal appearance of the brain, after coagulation, and had boiled the muscle previous to the examination; not being aware that this process must decompose red globules, should any exist, and cause the colouring matter to be separated. Boiling would also destroy any connecting medium by which the globules are united together; so that, if I may use the expression, there would only be the skeleton of a muscular fibre remaining to be examined.

Upon the present occasion, therefore, the fibres belonging to the fasciculi that compose the great muscle that lies upon the back of the bullock's neck, to raise the head, were selected, and were examined in 24 hours after the animal was killed; and we know that in all violent deaths, the muscular fibres continue capable of contraction beyond that period, after apparent death has taken place.

In this muscle the fasciculi are more loosely connected together than in almost any other animal body; and in the interstices between them there is no fat; but Mr. BAUER

found that in this recent state the fibres are held so firmly together by the mucus which surrounds them, and forms them into fasciculi, that it was only under water he could separate an integrant fibre for examination in the field of the microscope.

In its mechanism, he found it to correspond with the nervous fibre of a ganglion, differing only in the size of the globules, which were larger than those of the fibre in the ganglion in the proportion of $\frac{1}{2,000}$ parts of an inch to $\frac{1}{3,000}$ and $\frac{1}{4,000}$ parts.

The elastic transparent jelly uniting the globules together, had not the same elasticity as in the nervous fibre, so that it could not be drawn out from the contracted state to double its length without breaking.

The muscular fibre of a trout was treated in the same way, and the result was the same; the fibres were however more brittle than those in the bullock's neck.

From these facts, in addition to those communicated in the examination of the structure of ganglions, it is at last ascertained, that the structure of the fibres of nerves in general, and those peculiar to ganglions, as well as those that compose muscles, is so far the same, that they consist of single rows of globules united together by an elastic gelatinous transparent matter; they differ however in the size of the globules, and the degree of elasticity of the medium by which they are united; so that a less power will elongate a nerve than the fibres of a muscle, and to a greater extent, and it will restore itself with more velocity to a state of rest.

This structure of nerves and muscles, I consider to be

demonstrated in the annexed drawing; since I cannot believe Mr. BAUER has been led into any error upon this occasion; as no error has been detected in his microscopical observations for so many years continued, and the accuracy of his representations, of what he has seen, no one can doubt.

It is a curious confirmation of the acuteness of his eye, and the accuracy of his glasses, that LEUWENHOEK, who used a single microscope, and says it is the best that can be made, since the magnifying glass is the smallest speck that can be seen, declares a muscular fibre to be made of globules less than the red globules of the blood; and Dr. MONRO of Edinburgh, who published his microscopical observations on nerves and muscles, in the year 1783, made chiefly in the solar microscope, goes so far as to consider muscular fibres to be the continuation of nervous fibres, and gives an engraving of the mode in which the one terminates, or is lost in the other. Dr. MONRO, it is evident, had never seen a single fibre either of a nerve or muscle, only fasciculi of them, and found them so much alike as to be led to consider them the same. Both LEUWENHOEK and MONRO, from the want of a micrometer, were left to guess at relative dimension, and in such guesses were often very unsuccessful.

The globules in the nervous fibre being smaller than in the muscular, oversets MONRO's theory of their being the same; but that both authors, with means so very inadequate to those employed by Mr. BAUER, should have made such approaches to the truth, is highly creditable to them, and must prove highly satisfactory to Mr. BAUER, as well as to the public.

EXPLANATION OF PLATE II.

It represents muscular fibres magnified in different degrees.

Fig. 1. A fasciculus of fibres taken from the bullock's neck, sufficiently dissected to show the separate fibres; magnified 100 diameters.

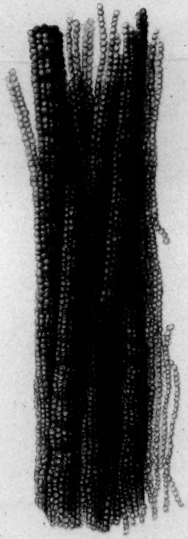
Fig. 2. A portion of the same fasciculus, the fibres more unravelled; magnified 200 diameters.

Fig. 3. A portion of a fibre in its contracted state, consisting of five blood globules, and exactly one inch in length, taken from the bullock's neck; magnified 400 diameters.

Fig. 4. The same length of fibre extended to its utmost, without giving way; magnified 400 diameters.

The calf was selected for this last purpose, the elastic medium between the globules being less brittle in the young than the full grown animal.

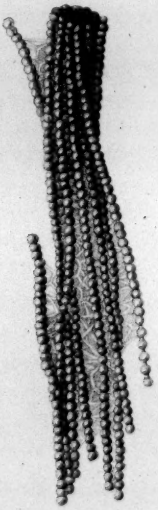
Fig. 1.



3.

4.

Fig. 2.



VI. *An account of the heat of July, 1825; together with some remarks upon sensible cold.* By W. HEBERDEN, *M. D. F. R. S.*

Read January 12, 1826.

As I think it may not improbably be deemed an object of some curiosity to the Royal Society to collect from different parts, an account of the very unusual heat of last July, I presume to offer the enclosed report of observations which I made at that time, at Datchet, in Buckinghamshire, with every precaution that suggested itself to me, to ascertain the real temperature of the atmosphere, uninfluenced by adventitious circumstances. The observations were made with a small sensible thermometer, which had been carefully graduated.

On Friday July 15, the wind blowing from the south-west, the thermometer was suspended in the shade of a large laburnum on my lawn, at a height of about five feet and a half from the ground. This tree was chosen, as admitting the air in some degree to pass through it, at a time when the wind and the sun were both in the same quarter. On the subsequent days, the wind being in the east and north-east, the thermometer was hung, at about the same height, from an external branch of a very thick Portugal laurel, standing likewise upon the lawn, at a distance from any building; where it was exposed to the full influence of the wind, and at the same time effectually sheltered either from the actual

rays of the sun, or from any object heated by it. I have been the more particular in giving this account, because it is often difficult, especially in great, or rapid changes of temperature, to get observations made with sufficient attention to avoid the neighbourhood of buildings, or other objects, which may considerably affect the thermometer, and give an appearance of heat sometimes greater, sometimes less than the truth.

In order to ascertain the greatest heat, the thermometer was repeatedly examined at short intervals during the hottest period of the day. The highest degrees indicated, under the circumstances I have detailed, were as follows:

July 15.	-	-	92° wind S. W.
17.	-	-	90°
18.	-	-	96° wind E.
19.	-	-	95°

The 16th is not set down, because, though it was a hot day, yet being several degrees cooler than the day preceding, it was not thought worth while to examine the thermometer with the same attention.

There is some reason to believe that an unusual degree of heat was felt about the same time, not only in most parts of Europe, but also in America.

The only instance of heat, that I am aware of having been noticed in England equal to this, was in July 1808; on the 13th of which month the thermometer is stated, in the Meteorological Journal of the Royal Society, to have risen to $93\frac{1}{2}^{\circ}$. On the 12th it had stood at 90°, and on the 14th it was 91°; the wind at the same time varying from east to

south and south-west. On the hottest of these days Mr. CAVENDISH's thermometer at Clapham rose to 96°.

It is true that other accounts have been recorded, some in the Transactions of this Society, of hot days, or hot seasons, which may have equalled, or possibly surpassed this: but till within the last sixty years, the use of the thermometer had hardly been understood sufficiently to enable one to rely upon the vague statements of earlier times.

To persons who may wish to compare this with the heat of tropical countries, it will not be thought superfluous to add, that the late Dr. HUNTER, whose accuracy is well-known to many members of the Royal Society, has stated in his valuable account of diseases in the West Indies, that the range of the thermometer at the hottest part of the day, and in the hottest season of the year, at Kingston, in Jamaica, is from 85° to 90°. In the coldest season it is about 5° lower.

It is not the least singular circumstance attending the heat of last July, that it should have subsided without rain, without lightning, without any change of wind, or any obvious cause; the succeeding days continuing dry and fair, as those before.

I am tempted to add to the above some other observations, which, if they are not immediately connected, are not entirely unconnected with this subject; for it cannot have escaped the attention of any person moderately conversant with natural philosophy, that the index of a thermometer is a very imperfect measure of what I may call the *sensible cold*, that is, of the degree of cold perceptible to the human body in its ordinary exposure to the atmosphere. For while the thermometer truly marks the temperature of the medium in

which it is placed, the sensations of the body depend altogether upon the rapidity with which its own heat is carried off. And this is by no means confined to the actual temperature of the air; but whatever alteration of quality increases its power of conducting heat; and, above all, whatever currents increase the succession of its particles in contact with the body, the same will increase the sensation of cold. Hence it is, that in very hot weather, the same stream of air which would heat a chamber, will nevertheless be cool to the feeling; on the other hand, when the thermometer was more than 80° below the freezing point, Captain PARRY observed, that while the air was still, the cold was borne without inconvenience.

It therefore occurred to me, that the proper way to estimate the *sensible cold*, would be, first to raise a thermometer to a height something exceeding the natural heat of the human body, and then to observe at what rate the quicksilver contracted upon exposure to the air. For this purpose I used a thermometer with a very small bulb, which might show the alteration of heat in a short time. This I held to the fire till it rose to about 120°, and then carried it in a warm glove into the open air. I had with me an assistant with a watch in his hand: and as soon as the mercury had descended to 100°, he began to count the seconds, while I continued to observe the thermometer, marking the degree of heat at the end of every ten seconds during half a minute. The result rather exceeded my own expectations; and (being, as far as I know, the only experiments of the kind,) I have thought the Society might not dislike to be made acquainted with them.

The circumstances that particularly engaged my attention were wind, and moisture. With these views the following experiments were made, and verified by repeated trials.

Experiment 1.

1821, January 3. A strong east wind. The temperature of the air 31°.

The thermometer in this, and all the experiments, being previously raised to 100°, in the manner before-mentioned, the descent of the mercury from that point was observed as follows:

After 10″ it was 78°. Decrement 22°
20″ —— 60° ——— 18°
30″ —— 52° ——— 8°

By the decrements, it is to be understood the descent in each successive ten seconds. This is added, because I consider it as the proper measure of the *sensible cold*, so long as the thermometer retains a heat approaching to that of the human body.

Experiment 2.

1821, Jan. 4. No perceptible wind. The temperature of the air 30°, the atmosphere hazy.

After 10″ therm. 89° Decrement 11°
20″ —— 80° ——— 9°
30″ —— 71° ——— 9°

Experiment 3.

1821, Feb. 10. A strong east wind. Temperature of air 47°. The atmosphere clear, with sunshine.

After 10″ therm. 82° Decrement 18°
20″ —— 73° ——— 9°
30″ —— 64° ——— 9°

Experiment 4.

1824, Jan. 9. A cold fog. No wind. Temperature of the air 37°.

After 10″ therm. 92° Decrement 8°
20″ —— 85° ——— 7°
30″ —— 79° ——— 6°

The most superficial view of these experiments shows the prodigious effect of wind to increase the *rate of cooling*, which, I apprehend, constitutes *sensible cold*; so that in experiment 3, though the thermometer suspended in the open air was 17° higher than in experiment 2, yet the *sensible cold* was very considerably greater; but when there was no wind, even a wet fog did not much, if at all, increase it. This, which at first sight may appear contradictory to experience is not, I believe, really so; for though the power of such air to carry off the heat of the body be indeed increased, yet so long as we remain at rest, we are in great measure unaffected by it;

so much the effect of wind exceeds that of mere moisture. It is by walking, or riding, in such a state of the atmosphere, that we produce on our bodies a current of moist air, which is then felt in proportion to the rapidity with which we pass through it. If it were thought worth while to bring this to the test of the thermometer, the instrument should be made to pass through the air at the same rate as the person would move.

VII. *On the transit instrument of the Cambridge Observatory; being a Supplement to a former Paper.* By ROBERT WOODHOUSE, *Esq. Plumian Professor of Astronomy in the University of Cambridge.*

Read January 19, 1826.

IN the brief account of the transit instrument which I had the honour some time ago of presenting to the Royal Society, I stated the circumstance of the instrument's deviation from the meridian arising from the unequal expansion of its braces; but no instance was then given of the magnitude of such deviation. I now subjoin one.

On the morning of Oct. 15, (civil reckoning) after observing the passage of Regulus, the southern shutters were accidentally left open, so that when I returned to observe the inferior culmination of the pole star, the sun was shining on the upper western brace, the object-glass of the instrument being towards the zenith. The effect of this was a retardation of more than 25 seconds in the star's passage, as will thus appear:

Rate of Clock —.17	Oct. 14.	$0^h\ 59^m\ 20^s$	Polaris.
		12 59 44	Polaris S. P.
	Oct. 15.	0 59 20	Polaris.
		12 59 17	Polaris S. P.
Reversed the axis.	Oct. 16.	0 59 19.5	Polaris.
	Oct. 17.	0 59 20	Polaris.
	&c.		

I now view, with great suspicion, all the observations of the sun's transits, which I observed previously to the detection of that source of inequality which is the subject of the present, and of my former communication.

VIII. *Account of a series of observations, made in the summer of the year* 1825, *for the purpose of determining the difference of meridians of the Royal Observatories of Greenwich and Paris; drawn up by* J. F. W. Herschel, *Esq. M. A. Sec. R. S. Communicated by the Board of Longitude.*

Read January 12, 1826.

Operations having been carried on to a considerable extent in France, and other countries on the continent, for the purpose of ascertaining differences of longitude by means of signals, simultaneously observed at different points along a chain of stations; and the Royal Observatory at Paris, in particular, having been connected in this manner with a number of the most important stations, it was considered desirable by the French government that the Royal Observatory at Greenwich should be included in the general design. The British Board of Longitude was accordingly invited to lend its co-operation towards carrying into effect a plan for that purpose; and the invitation being readily accepted on their part, I was deputed, in conjunction with Capt. Sabine, in the course of the last summer, to direct the practical details of the operation on the British side of the channel, and to make the necessary observations. Every facility was afforded us in making our dispositions, on the part of the different branches of His Majesty's government to which it was found necessary to apply. A detachment of artillery was placed, by his Grace the Duke of Wellington, Master

General of the Ordnance, under the orders of Capt. SABINE. Horses, waggons, and men, were furnished for the conveyance of a tent, telescopes, rockets, and other apparatus; and four of the chronometers belonging to the Board of Admiralty were placed at our disposal. The rockets required for making the signals were furnished us from France. It would have been easy, doubtless, to have procured them from the Royal Arsenal at Woolwich; but on the representation of Colonel BONNE, to whom the principal direction of the operations in France was intrusted, it was thought more advisable to accept an offer made to us of any number which might be required, prepared at Paris expressly for similar operations, carrying a charge of 8 ounces of powder, the instantaneous explosion of which, at their greatest altitude, was to constitute the signals to be observed.

Our previous arrangements being made, on the 7th of July I left London; and after visiting the station pitched upon at Wrotham, which was the same with that selected by Capt. KATER and Major COLBY, as a principal point in their triangulation in 1822; and finding it possessed of every requisite qualification for the purpose of making the signals, from its commanding situation, being unquestionably the highest ground between Greenwich and the coast, proceeded to Fairlight Down, near Hastings, where I caused the very convenient observatory tent, belonging to the Board of Longitude, to be pitched immediately over the centre of the station of 1821, which was readily found from the effectual methods adopted by the gentlemen who conducted the trigonometrical operations in that year, for securing this valuable point. Here, on the 8th, I was joined by Capt.

SABINE, who, it had been arranged, should proceed to the first observing station on the French side of the Channel, there to observe, in conjunction with Colonel BONNE, the signals made on the French coast, and those made at the station of Mont Javoult; which latter were to be observed immediately from the observatory at Paris; while, on the other hand, it was agreed that M. le Lieutenant LARGETEAU, of the French corps of geographical engineers, should attend at Fairlight, on the part of the French commission, and observe, conjointly with myself, the signals made at La Canche, the post on the opposite coast (elevated about 600 feet above the sea, being nearly the level of Fairlight Down) and also those to be fired from Wrotham Hill, which were expected to be immediately visible from a scaffold, raised for the purpose on the roof of the Royal Observatory of Greenwich. By this arrangement, and by immediate subsequent communication of the observations made at each station, it was considered that the advantage of two independent lines of connexion, a British and a French, would be secured between the two extreme stations; i. e. the two national observatories; every possibility of future misunderstanding obviated, and all inconvenience on either side, arising from delay, or miscarriage in the transmission of observations, be avoided.

With the assistance of Capt. SABINE, and by the help of exact information as to the azimuths of Wrotham and other nearer stations in the triangulation of 1821, with which Capt. KATER had obligingly furnished us, and of which Fairlight Church proved the most convenient, being close at hand and favorably situated, and easily visible in the twilight; and

from the previously calculated azimuth of La Canche (114° 30′ E.); four night glasses by DOLLOND, provided at the order of the Board of Longitude expressly for this operation, and which I had caused to be fixed on posts firmly driven into the ground beneath the tent, were then pointed, two on the station of La Canche, and two on that of Wrotham Hill. Those directed to the former were of four inches clear aperture, the others of three. In case of any difficulty arising as to the pointing, I had taken care to provide myself with an excellent eight-inch repeating theodolite, on the REICHENBACH construction, by SCHENCK, of Berne; but it was found unnecessary to use it, as the night glasses were purposely constructed with an azimuthal motion, and a rough graduation read off by an adjustable vernier, so as to allow their being set at once a few minutes before the observations commenced, by taking Fairlight steeple as a zero point; a circumstance which proved exceedingly convenient, as it allowed of their being dismounted after each night's observations, and removed to a place of security; and thus rendering it unnecessary to harass our small party by keeping guard in our absence.

On the night of the 8th I had directed blue lights to be fired at Wrotham, as a trial of the visibility of the stations, or rather as a verification of the pointing of the telescopes; for on the former point there could be no doubt, the station at Wrotham being situated precisely on the edge of the escarpment of the chalk which borders the Weald of Kent, and having been actually connected with Fairlight by direct observation, while no obstacle but a low copse wood, over which it might fairly be presumed that no rocket would

fail to rise, separated it from a direct view of Greenwich, at about 20 miles distance. Either from haze in the atmosphere, or from the too great distance, nothing was seen that night or the next; which however caused no uneasiness, as we could depend on our instruments and information. The next morning Capt. SABINE quitted Hastings, and joined Col. BONNE, at his post, on the morning of the 10th, the day appointed for the commencement of the observations; meanwhile I was joined by M. LARGETEAU, who remained with me the whole time of their continuance, performing every part of a most scrupulous and exact observer, as the observations herewith communicated will abundantly testify.

The observations were continued during 12 nights, 10 signals being made at each rocket station every night. The weather throughout the whole of this time was magnificent, and such as is not very likely to occur again for some years; a circumstance of the last importance in operations of this nature, where lights are to be seen across nearly 50 miles of sea, and also by reason of the verification of the sidereal times at the observatories by transits. One night only a local fog deprived us of the sight of 13 out of the 20 signals; but on the whole, out of 120 made at Wrotham, no less than 112 were seen from Fairlight (about 40 miles) and 89 from Greenwich; while out of the same number made at La Canche, 93 were observed at the former post. I am sorry to add, however, that owing to a combination of untoward circumstances, which no foresight or exertion on the part of Capt. SABINE or myself could possibly have led us to calculate on, or enabled us to prevent, and which the most zealous endeavours on that of Col. BONNE failed to remedy, no

less than eight out of the twelve nights' observations were totally lost, as to any result they might have afforded, and the remainder materially crippled; so that a much more moderate estimate of the value of our final result must be formed, than would otherwise have been justified. Still it is satisfactory to be able to add, (such is the excellence of the method) that a result on which considerable reliance can be placed, may be derived from the assemblage of the observations of these four nights; and when it is stated that this result appears not very likely to be a tenth of a second in error, and extremely unlikely to prove erroneous to twice that amount, it will perhaps be allowed that, under such circumstances, more could hardly be expected.

I. *Observations made at the Royal Observatory at Paris.*

Station de l'Observatoire Royal. — Feux de Mont-Javoult.

Jours. Juillet 1772.	No. des Signaux.	Apparition des Signaux. Observées en tems de la Pendule.	Apparition des Signaux. En tems Sideral.	Noms des Observateurs.	Avance Pendule sur le tems Sideral.	Remarques.
		A*	B			
☾	7	18h 15′ 52″.0	18h 15′ 40″.3	Mathieu	- -	brillant élevé.
		52.2	40.5	Savary		
		52.0	40.3	Nicollet		
	8	26 17.5	26 5.8	Mathieu	- -	excessivement faible : observation douteuse
18	9	35 52.9	35 41.2	Mathieu	11″.7	très brillant, assez élevé.
		53.1	41.4	Savary		
		52.5	40.8	Nicollet		
	10	45 56.0	45 44.3	Mathieu	- -	faible, peu élevé.
		55.9	44.2	Savary		
		55.6	53.9 †	Nicollet		† So in the original. (H.)
☌	1	17 19 49.8	17 19 37.0	Mathieu	- -	brillant.
		49.8	37.0	Savary		
	2	29 42.4	29 29.6	Mathieu	- -	faible, élevé.
	5	59 44.4	59 31.6	Mathieu	- -	assez brillant, peu élevé.
		44.6	31.8	Savary		
19	6	18 9 58.1	18 9 45.3	Mathieu	12.8	assez brillant, peu élevé.
		58.1	45.3	Savary		
	8	29 54.3	29 41.5	Mathieu	- -	peu brillant.
		54.4	41.6	Savary		
	9	40 5.5	39 52.7	Mathieu	- -	brillant, très élevé.
		5.1	52.3	Savary		
	10	49 56.3	49 43.5	Mathieu	- -	brillant, très élevé.
		56.1	43.3	Savary		
☿	2	17 33 50.7	17 33 36.5	Mathieu	- -	très faible, observation très douteuse.
	3	43 45.5	43 31.3	Mathieu	- -	brillant, assez élevé.
		45.5	31.3	Savary		
	4	53 49.8	53 35.6	Mathieu	- -	très faible, bas.
	5	18 3 46.7	18 3 32.5	Mathieu	- -	très brillant, très élevé.
		46.8	32.6	Savary		
20	6	13 48.6	13 34.4	Mathieu	14.2	assez brillant.
		48.1	33.9	Savary		
	7	23 49.9	23 35.7	Mathieu	- -	peu brillant, assez élevé.
		49.7	35.5	Savary		
	8	33 53.3	33 39.1	Mathieu	- -	peu brillant, assez élevé.
		53.6	39.4	Savary		
	9	43 56.3	43 42.1	Mathieu	- -	brillant, très élevé.
		56.4	42.2	Savary		
	10	53 54.2	53 40.0	Mathieu	- -	brillant, très élevé.
		54.6	40.4	Savary		

* La colonne (A) renferme les nombres qui ont été trouvés par les observations des feux. La colonne (B) renferme les nombres de la colonne (A) corrigés de l'avance de la pendule. Les nombres de la colonne (B) sont ceux qui doivent être comparé au tems sideral absolu de Greenwich.

Observations made at the Royal Observatory at Paris.

Station de l'Observatoire Royal. — Feux de Mont-Javoult.

Jours, Juillet 1788.	No. des Signaux.	Apparition des Signaux. Observées en tems de la Pendule.	En tems Sideral.	Noms des Observateurs.	Avance Pendule sur le tems Sideral.	Remarques.
		A	B			
21	1	17h 27′ 43.″2	17h 27′ 26.″8	Mathieu	- -	peu brillant, peu élevé.
		43.9	27.1	Savary		
	2	37 39.5:	37 23.1:	Mathieu	- -	brillant, très élevé, j'ai vu une trainée lumineuse de 37″ à 40″ j'estime le grand éclat vers 39″.5.
	3	47 48.3	47 31.9	Mathieu	- -	assez brillant, très élevé.
		48.7	32.3	Savary		
	4	57 42.3	57 25.9	Mathieu	- -	faible, peu élevé.
		59.6	43.2	Savary	- -	autre feu brillant et très élevé.
	5	18 7 37.5	18 7 21.1	Savary	- -	très faible, bas.
21		7 57.3	7 40.9	Mathieu	16″.4	autre feu assez brillant et élevé.
		57.4	41.0	Savary		
	6	17 41.3	17 24.9	Savary	- -	faible et peu élevé.
		17 46.8	17 30.4	Mathieu	- -	autre feu assez brillant, élevé.
		46.6	30.2	Savary		
	7	27 46.5	27 30.1	Mathieu	- -	assez brilliant et élevé, explosion non instantanée.
		46.4	30.0	Savary		
	8	37 51.6	37 35.2	Savary	- -	faible et bas.
		37 57.2	37 40.8	Mathieu	- -	très brillant et très élevé.
		57.1	40.7	Savary		
	10	57 56.7	57 40.3	Savary	- -	premier feu, assez brillant, mais bas.
		58 0.3	57 43.9	Mathieu	- -	autre feu, très brillant, assez élevé.
22	1	17 31 29.9	17 31 12.3	Mathieu	- -	très brillant, très élevé.
		29.6	12.0	Savary		
	2	41 29.3	41 11.7	Mathieu	- -	assez brillant, peu élevé.
		29.3	11.7	Savary		
	3	51 36.2	51 18.6	Mathieu	- -	assez brillant et élevé.
		36.4	18.8	Savary		
	4	18 1 33.3	18 1 15.7	Mathieu	- -	assez brillant et élevé.
		33.2	15.6	Savary	17.6	
	5	11 39.4	11 21.8	Mathieu	- -	très brillant et assez élevé.
		39.2	21.6	Savary		
	6	22 1.3	21 43.7	Mathieu	- -	assez brillant et élevé.
		1.1	43.5	Savary		
	7	31 49.3	31 31.7	Mathieu	- -	assez brillant et assez élevé.
		49.5	31.9	Savary		
	9	51 47.5	51 29.9	Mathieu	- -	assez brillant et assez élevé.
		47.3	29.7	Savary		

On a observé les signaux de feu donnés à Mont-Javoult près de Gisors dans un petit cabinet situé dans la partie supérieure de l'observatoire. Les lunettes dont on se servait étaient très près d'une pendule que j'avais placée dans ce cabinet; ensorte que l'on pouvait aisement prendre la seconde et la compter par le moyen des battemens du balancier, qui s'éntendaient parfaitement. Après l'observation des signaux je comparais, à l'aide d'un chronomètre, la pendule à celle qui est en bas à côté de la lunette meridienne. Ces comparaisons m'ont donné pour chaque jour l'avance de la pendule des feux sur celle de la lunette méridienne et par suite sur le tems sideral. Je me suis attaché à régler la pendule, qui est près de la lunette méridienne par les passages durant le jour des sept étoiles suivantes: Aldebaran, La Chèvre, Rigel, α Orion, Arcturus, α Couronne, α Serpent. J'ai observé 5 de ces étoiles le 18, 3 le 19, 7 le 20, 4 le 21, et 5 le 22. J'ai calculé leurs positions apparentes d'après les *positions moyennes et les corrections in Right Ascension* données par Mr. SOUTH.

L. MATHIEU.

II. *Captain* SABINE'S *observations at Lignieres.*

Chronometer of Motel, No. 39.

Observations du 18 Juillet, huitième jour.

	Apparition des Signaux, en tems de la montre. à l'Orient.	à l'Occident.	Remarques.
	h. min. sec.	h. min. sec.	
1			
2			
3	9 49 33·4	9 54 52,	Signal de La Canche [faible.
4	9 59 34,0	non vu.	
5	10 09 37,2	10 14 54,du	Signal de La Canche [très faible.
6	10 19 33,6	non vu.	
7	10 29 34,4	id.	
8	non vu.	id.	
9	10 49 32·8	id.	
10	10 59 33.6	id.	

Le Colonel BONNE à Mont-Javoult, moi seul.
Les signaux de Mont-Javoult bien vus, excepté le 7me qui était faible.

Observations du 19 Juillet, le neuvième jour.

	Apparition des Signaux, en tems de la montre. à l'Orient.	à l'Occident.	Remarques.
	h. min. sec.	h. min. sec.	
1			
2	9 39 30.4	fa. du.	
3	- -	9 44 50	
4			
5	10 09 39,6	10 14 50.4	———
6			
7	- -	10 34 49.6	
8			
9	10 49 41,2	10 54 53,6	
10	10 59 30,0	11 05 01,4	

Observations du 20 Juillet, le dixième jour.

	Apparition des Signaux, en tems de la montre. à l'Orient.	à l'Occident.	Remarques.
	h. min. sec.	h. min. sec.	
1			
2			
3	9 49 39·6		
4			———
5	10 09 27·6		
6			
7	10 29 27·2		
8			
9	10 49 30·5	10 54 47·6	faible.
10			

Observations du 21 Juillet, le onzième jour.

	Apparition des Signaux, en tems de la montre. à l'Orient.	à l'Occident.	Remarques.
	h. min. sec.	h. min. sec.	
1	- -	9 34 50.8	
2	9 39 24·8		
3	9 49 32·8	9 54 50·4	
4	- -	10 04 53·2	
5	10 09 38·4	10 14 51.2	Le 6me signal de Mont-Javoult rasant l'horizon.
6	10 19 26·4	- -	
7	- -	10 34 49·6	
8	10 39 33·2	10 44 59·4	
9			
10	10 59 33·2	11 04 52,0	

Captain SABINE'S *observations of signals seen from Lignieres.*

Observations du 22 Juillet, 12me jour.

	Apparition des Signaux, en tems de la montre.		Remarques.
	à l'Orient.	à l'Occident.	
	h. min. sec.	h. min. sec.	
1	9 29 18·6	9 34 55·6	
2	- -	9 44 50·8	
3	9 49 22,0	9 54 53·6	
4	9 59 17,2	10 04 53,2	
5	10 09 22,:	10 15 08,8	———
6	10 19 41·,6	10 24 48,4	
7	10 29 28,6	10 34 58,8	
8	- -	10 44 57,6	
9	Eclair.	10 54 48,0	
10	Eclair.	11 04 48,8	

III. *Colonel* BONNE'S *observations of signals seen from Lignieres.*

Chronometer Motel, No. 39.

Observations du 19 Juillet, le neuvième jour.

	Apparition des Signaux, en tems de la montre.		Remarques.
	à l'Orient.	à l'Occident.	
	h. min. sec.	h. min. sec.	
1			
2	- -	9 44 49·4	
3	- -	9 54 49·8	
4			
5	10 09 39·4	10 14 50,4	———
6			
7	- -	10 34 49·8	
8			
9	10 49 41,0	10, 54 53,2	
10	10 59 30,8	11 05 01,0	

Observations du 20 Juillet, le dixième jour.

	Apparition des Signaux, en tems de la montre.		Remarques.
	à l'Orient.	à l'Occident.	
	h. min. sec.	h. min. sec.	
1			
2			
3	9 49 29,6		
4			
5	10 09 28,0	———	———
6			
7			
8			
9	10 49 31,2		
10			

Colonel BONNE'S *observations of signals seen from Lignieres.*

Observations du 21 Juillet, le 11me jour.

	Apparition des Signaux, en tems de la montre. à l'Orient.	à l'Occident.	Remarques.
	h. min. sec.	h. min. sec.	
1	- -	9 34 50,8	
2	9 39 24,6		
3	9 49 32,6		
4	- -	10 04 53,0	———
5	10 09 38,8		
6			
7	- -	10 34 49,6	
8	- -	10 44 59,4	
9			
10	10 59 33,4	11 04 51,6	

Observations du 22 Juillet, le 12me jour.

	Apparition des Signaux, en tems de la montre. à l'Orient.	à l'Occident.	Remarques.
	h. min. sec.	h. min. sec.	
1	9 29 16,4	9 34 55,4	
2			
3	9 49 21,8	9 54 53,4	
4	9 59 17,0	10 04 53,	———
5	10 09 21,4	10 15 08,4	
6	10 19 41,8	10 24 48,2	
7	10 29 28,2	10 34 58,6	
8	- -	10 44 57,4	
9	10 49 23,	10 54 47,4	
10	10 59 25,8	11 04 48,8	

IV. *Observations of the signals at the Fairlight Station, by Mr.* HERSCHEL.

By BAKER'S Chronometer, No. 744. Going M. T. beating half seconds.

First Day's Observations, July 11, 1825.

No.		h. m. s.	Remarks.
1	La Canche	———	
	Wrotham	- -	Seen, but the time not seized correctly.
2	La Canche	———	
	Wrotham	9 41 7·6	The train began at $9^h\,41^m\,2^s$.
3	La Canche	———	
	Wrotham	9 51 3·5	Train began at $9^h\,20^m\,59^s{\cdot}5$.
4	La Canche	9 56 23·2	Seen by the gunners with naked eye.
	Wrotham	10 1 4·8	Very good.
5	La Canche	10 5 59·2	Faint and indistinct. Seen by the gunners.
	Wrotham	10 11 3·0	Precise.
6	La Canche	———	
	Wrotham	10 21 4·4	Train began at $59^s{\cdot}0$.
7	La Canche	———	
	Wrotham	10 31 4·7	Train began at $58^s{\cdot}5$.
8	La Canche	10 36 15·8	Very faint. Not seen by the men.
	Wrotham	9 41 6·3	
9	La Canche	10 46 12·4	Seen by the men.
	Wrotham	10 51 3·3	
10	La Canche	———	
	Wrotham	11 1 8·2	

Second Day's Observations, July 12, 1825.

No.		h. m. s.	Remarks.
1	La Canche	9 26 24·5	
	Wrotham	9 36 7·3	Not the true explosion according to M. LARGETEAU.
2	La Canche	9 36 23·3	Very bright and sharp.
	Wrotham	9 41 7·5	
3	La Canche	———	Lost by looking the wrong way.
	Wrotham	———	
4	La Canche	9 56 17·8	Distinct.
	Wrotham	10 1 8·8	Sharp and bright.
5	La Canche	10 6 31·7	Extremely faint. Doubtful.
	Wrotham	10 11 9·2	Bright.
6	La Canche	10 16 23·6	Distinct.
	Wrotham	10 21 10·0	Bright.
7	La Canche	10 26 22 5	Seen by Mr. GILBERT with naked eye.
	Wrotham	10 31 8·0	Bright.
8	La Canche	10 36 21·9	
	Wrotham	10 41 8·1	Train began at 1^s.
9	La Canche	10 46 16·5	? $15^s{\cdot}5$. The decimal correct.
	Wrotham	10 51 5·4	Train began at $0^s{\cdot}5$.
10	La Canche	10 56 16·4	Very bright. Train seen 4 or 5^s.
	Wrotham	11 1 6·6	Train began at $0^s{\cdot}5$.

Mr. HERSCHEL'S *observations of the signals seen from Fairlight.*

Third Day, July 13, 1825.

No.		h. m. s.	Remarks.
1	La Canche	———	A thick sea-fog suddenly came on 2^m before the time, though perfectly clear till then.
	Wrotham	9 31 6·0::	A mere suspicion. Fog thicker.
	La Canche	———	Fog.
2	Wrotham	———	Fog.
	La Canche	———	Fog.
3	Wrotham	9 51 12·5	Very faint, but distinct. Fog clear-
	La Canche	9 56 27·6	Distinct. [ing.
4	Wrotham	———	Object-glasses examined. All covered with moisture from the fog.
	La Canche	10 6 28·5	Well observed. Train seen.
5	Wrotham	10 11 16·5	Perfectly well seen.
	La Canche	10 16 18·1	
6	Wrotham	10 21 14·1	Well seen; but the glass dim, and the fog coming on again.
	La Canche	———	Fog suddenly came on again, and
7	Wrotham	———	is surprisingly dense, so as
	La Canche	———	scarcely to allow the Mill to be
8		———	seen; yet the stars are clear to within 10 degrees of the horizon.
	Wrotham	———	Fog.
9	La Canche	———	Fog.
	Wrotham	———	Fog.
10	La Canche	———	Fog.

Fourth Day, July 14, 1825.

No.		h. m. s.	Remarks.
1	La Canche	9 26 22·7	Very distinct; train seen.
	Wrotham	9 31 15·1	A pretty strong breeze.
	La Canche	9 36 15·3	Train perfectly well seen.
2	Wrotham	9 41 16·4	
	La Canche	9 46 28·1	Train seen.
3	Wrotham	9 51 15·2	
	La Canche	9 56 17·4	Train seen. Wind increasing.
4	Wrotham	10 1 15·4	
	La Canche	10 6 21·5	
5	Wrotham	10 11 19·6	
6	La Canche	10 16 24·5	? 23″·5—am almost sure 23″·5 is
	Wrotham	10 21 18·8	the right.
	La Canche	10 26 25·0	
7	Wrotham	10 31 15·2	Exploded irregularly at half its height.
	La Canche	10 36 17·5	Train *not* seen. N. B. A star in
8	Wrotham	10 41 18·5	the field of the glass.
	La Canche	10 46 27·8	First a bright spark; then the
9	Wrotham	10 51 16·6	train; then long after, a feeble explosion at 27″·8. The first flash was brighter than the explosion.
	La Canche	10 56 19·7	Train feebly seen.
10	Wrotham	11 1 17·8	

Fifth Day, July 15, 1825.

No.		h. m. s.	Remarks.
	La Canche	9 26 30·4	The first flash seen at 19″·4 on
1			lighting the rocket. The flash
	Wrotham	9 31 22·3	at 30″·4 very bright.
2	La Canche	9 36 23·4	Fainter than the 1st flash of No. 1.
	Wrotham	9 41 23·2	A slight flash at lighting. The rocket did not rise.
	La Canche	9 46 24·2	A flash at 16″·3 low down. The
3			flash at 24″·2 higher, and to the
	Wrotham	———	right of the former. (The telescope inverts. N. B.)
	La Canche	9 56 25·2	Faint, but very distinct.
4	Wrotham	10 1 18·0::	
	La Canche	10 6 23·5	
5	Wrotham	10 11 23·4	Signal regular and distinct, but observation uncertain from a violent noise in the adjoining field.
	La Canche	10 16 18·1	Sharp and good, but low.
		10 16 25·2	Feeble and high, to the right of the former.
6	Wrotham	10 21 22·0	Certainly ·0, but the second uncertain, from a violent noise which drowned the beat of the watch.
	La Canche	10 26 22·5	Noise continued, and the ob-
7	Wrotham	10 31 25·9	servations uncertain on account of it.
8	La Canche	10 36 25·3	Single explosion; well observed.
	Wrotham	10 41 23·3	Well observed.
	La Canche	10 46 25·4	Single explosion; extremely f.
9	Wrotham	10 51 23·4	Well observed.
	La Canche	10 56 21·5	
10	Wrotham	———	The train seen. No explosion. The signal not repeated.

Sixth Day, July 16, 1825.

No.		h. m. s.	Remarks.
	La Canche	9 26 26·3	Extremely faint.
1	Wrotham	9 31 31·4	The decimal correct, the second possibly erroneous from noise.
	La Canche	9 36 19·1	Small bright spark.
2		24·3	Broad feeble flash, higher, and to the apparent right
	Wrotham	9 41 30·0	Exact on the beat.
	La Canche	9 46 38·0	Single bright flash.
3	Wrotham	9 51 26·1	Explosion distinct but unexpected, as it happened before the rocket reached its greatest
	La Canche	———	elevation.
4	Wrotham	10 1 31·6	Regular, and well observed.
	La Canche	———	
5	Wrotham	10 11 30·8	Regular, and well observed.
	La Canche	10 16 32·2	Bright single flash.
6	Wrotham	10 21 32·7	
	La Canche	10 26 21·5	Excessively faint.
7	Wrotham	10 31 29·1	
	La Canche	10 36 27·2	Extremely faint.
8	Wrotham	10 41 35·9	
	La Canche	10 46 18·2	Very bright.
9	Wrotham	10 51 29·9	
	La Canche	10 56 29·6	Very bright. Observed with M.
10	Wrotham	11 1 29·7	LARGETEAU'S glass; a doubt having arisen as to its correct pointing, he having seen none of the La Canche signals this evening.

Mr. HERSCHEL'S *observations of the signals seen from Fairlight.*

Seventh Day, July 17, 1825.

No.		h.	m.	s.	Remarks.
1	La Canche	9	26	20·6	
	Wrotham	9	31	36·3	
2	La Canche	9	36	20·5	Excessively faint but instantane-
	Wrotham	9	41	35·9	[ous.
3	La Canche	9	46	27·0	: : A mere suspicion.
	Wrotham	9	51	37·2	
4	La Canche	9	56	28·0	Well observed.
	Wrotham	10	1	37·6	
5	La Canche	10	6	28·2	Telescope put in focus by a *.
	Wrotham	10	11	35·7	
6	La Canche	10	16	29·5	Extr. faint, like a * of 10 m.
	Wrotham	10	21	38·9	Exactly observed.
7	La Canche	10	26	27·8	Very distinct; perfectly well ob-
	Wrotham	10	31	41·0	[served.
8	La Canche	10	36	24·5	: : A pretty strong suspicion.
	Wrotham	10	41	41·1	
9	La Canche	10	46	38·5	The second doubtful, owing to the lateness of the explosion.
	Wrotham	10	51	31·7	
10	La Canche	10	56	30·2	: : : A faint suspicion.
	Wrotham	11	1	38·7	

Eighth Day, July 18, 1825.

No.		h.	m.	sec.	Remarks.
1	La Canche				
	Wrotham	9	31	41·9	Good.
2	La Canche	9	36	30·9	Good.
	Wrotham	9	41	46·0	Good.
3	La Canche	9	46	29·7	Good.
	Wrotham	9	51	49·5	Good.
4	La Canche	9	56	32·8	Good.
	Wrotham	10	1	50·3	Good.
5	La Canche	10	6	31·4	Good.
	Wrotham	10	11	48·6	Good. Uncommonly bright.
6	La Canche	10	16	32·3	Good.
	Wrotham	10	21	47·0	Good.
7	La Canche	10	26	23·4	Unexpected; possibly 1' wrong.
	Wrotham	10	31	42·8	Good.
8	La Canche				
	Wrotham	10	41	47·2	
9	La Canche	10	46	27·0	: : Ill observed.
	Wrotham	10	51	43·1	
10	La Canche	10	56	24·9	Perfectly well observed.
	Wrotham	11	1	42·0	

Ninth Day, July 19, 1825.

No.		h.	m.	s.	Remarks.
1	La Canche	9	26	31·3	or 34·3, certainly one or the other.
	Wrotham	9	31	51·5	Very brilliant.
2	La Canche	9	36	33 0	Very bright; well observed.
	Wrotham	9	42	0·5	Remained extremely long in the air, & mounted to a vast height.
3	La Canche	9	46	33·8	
	Wrotham	9	51	53·8	
4	La Canche	9	56	29·1	
	Wrotham	10	1	56·4	
5	La Canche				Missed by looking into the wrong telescope by mistake.
	Wrotham	10	11	51·1	Burst without rising.
	La Canche	10	16	32·2	Train seen before the flash.
6	Wrotham	10	22	2·5	Two rockets fired. The first burst, the second observed as here set down.
7	La Canche	10	26	33·7	Train seen as well as flash.
	Wrotham	10	32	24·8	
8	La Canche	10	36	35·0	Extremely faint; the train as bright as the flash.
	Wrotham	10	41	59·7	Very bright.
	La Canche	10	46	37·6	Train seen.
9	Wrotham	10	51	59·8	Mounted to an immense height.
	La Canche	10	56	29·5	The first flash at lighting observed; a second flash a long while after, seen, but time not taken.
10	Wrotham	11	1	51·0	First flash, rocket burst.
			2	3·5	Second rocket, rose regularly.

Tenth Day, July 20, 1825.

No.		h.	m.	s.	Remarks.
1	La Canche				
	Wrotham	9	31	43·3	
2	La Canche	9	36	36·2	
	Wrotham	9	41	58·0	
3	La Canche	9	46	37·9	Single flash.
	Wrotham	9	51	56·0	
4	La Canche				
	Wrotham	10	1	56·7	
5	La Canche	10	6	44·1	Single p. bright flash.
	Wrotham	10	11	57·9	Single flash; train not seen.
6	La Canche				
	Wrotham	10	22	4·4	
7	La Canche				
	Wrotham	10	31	58·1	
8	La Canehe				
	Wrotham	10	41	58·0	A second fired, but both were bad signals. Observation of little value.
9	La Canche				
	Wrotham	10	51	58·0	Large flash; some seconds after a small faint one.
10	La Canche				
	Wrotham	11	1	58·5	

Mr. HERSCHEL'S *observations of the signals seen at Fairlight.*

Eleventh Day, July 21, 1825.

No.		h. m. sec.	Remarks.
1	La Canche	———	A most favourable night, and transparent atmosphere.
	Wrotham	———	Three rockets fired, but all burst, and none could be observed.
2	La Canche	9 36 36·1	
	Wrotham	9 42 7·7	Good. The rocket rose regularly.
	La Canche	9 46 39·0	Excellent.
3	Wrotham	9 52 2·5	Three rockets fired in close succession, all burst.
		3·0	
		3·5	
	La Canche	9 56 41·5	
4	Wrotham	10 2 2·7	or 1·7 Both burst without rising.
		8·0	
	La Canche	10 6 39·8	
5	Wrotham	10 12 1·0	Both well observed, but both burst without rising.
		8·9	
6	La Canche	10 16 37·7	Excessively feeble, but certain.
	Wrotham	10 22 1·5	Both burst without rising.
		8·8	
	La Canche	10 26 28·3	
7	Wrotham	10 — —	Two fired; the first missed; both burst.
		32 8·4	
8	La Canche	10 36 47·9	Very good; train seen; the rocket remained very long in the air.
	Wrotham	10 42 1·1	Both burst.
		2·0	
	La Canche	10 46 41·9	
9	Wrotham	10 52 2·5	Both burst.
		22·0	
	La Canche	10 56 41·1	
10	Wrotham	11 2 1·0	One only, which burst; being the 19th out of 20 fired to night.

Twelfth Day, July 22, 1825.

No.		h. m. s.	Remarks.
1	La Canche	9 26 35·5	Very bright and fine.
	Wrotham	9 32 9·0	Regular and good.
2	La Canche	9 36 39·8	Train well seen.
	Wrotham	9 42 7·0	Regular and well observed.
	La Canche	9 46 42·5	Train seen. Rose to a vast height.
3	Wrotham	9 52 8·6	Regular and well observed.
	La Canche	9 56 42·5	Very good.
4	Wrotham	10 2 10·1	Perfect observation.
	La Canche	10 6 58·0	Very exact.
5	Wrotham	10 12 0·7	Burst without rising.
	La Canche	10 26 48·2	
6	Wrotham	10 32 3·0	Two fired; both burst.
		8·9	
	La Canche	10 26 48·2	
7	Wrotham	10 32 3·0	Both burst.
		8·9	
	La Canche	10 36 47·0	
8		58·8	Rose regularly, but rather a doubtful observation.
	Wrotham	10 42 1·0	
		7·2	All three burst.
		—	
9	La Canche	10 46 36·9	Single. Train seen.
	Wrotham	10 52 2·0	
		8·7	Both burst.
	La Canche	10 56 38·0	
10		43·5	:: Doubtful.
	Wrotham	11 2 2·8	Burst.

V. *Copie des Observations à Fairlight Down par* C. L. LARGETEAU.

1825. (BAKER'S Chronometer, N°. 744.)

12 Juillet.

No.		h. m. sec.	Remarks.
1	La Canche	———	
	Wrotham	———	
2	La Canche	9 36 22·9	
	Wrotham	9 41 7·9	
3	La Canche	———	
	Wrotham	9 51 7·0	
	La Canche	9 56 17·8	
4	Wrotham	10 1 8·6	
	La Canche	10 6 31·7	
5	Wrotham	10 11 9·4	
6	La Canche	10 16 23·8	
	Wrotham	10 21 10·0	
7	La Canche	10 26 22·5	
	Wrotham	10 31 7 9	
8	La Canche	10 36 21·9	
	Wrotham	10 41 8·3	
9	La Canche	10 46 16·5	
	Wrotham	10 51 5·1	
10	La Canche	10 56 15·9	
	Wrotham	11 1 11·7	il faut peut être $11^h\ 1^m\ 6.7^s$

13 Juillet.

No.		h. m. s.	Remarks.
1	La Canche	———	
	Wrotham	———	
2	La Canche	———	
	Wrotham	———	
3	La Canche	———	
	Wrotham	———	
4	La Canche	9 56 27·3	
	Wrotham	———	
5	La Canche	10 6 28·5	———
	Wrotham	10 11 14·6	
6	La Canche	10 16 17·9	
	Wrotham	10 21 14·0	
7	La Canche	———	
	Wrotham	———	
8	La Canche	———	
	Wrotham	———	
9	La Canche	———	
	Wrotham	———	
10	La Canche	———	
	Wrotham	———	

M. Largeteau's *Observations at Fairlight continued.*

14 Juillet.

No.		h. m. s.	Remarques.
1	La Canche	9 26 22·7	
	Wrotham	9 31 15·3	
2	La Canche	———	
	Wrotham	9 41 16·6	
3	La Canche	9 46 28·0	
	Wrotham	9 51 15·0	
4	La Canche	9 56 17·4	
	Wrotham	10 1 15·4	
5	La Canche	10 6 21·8	
	Wrotham	10 11 19·5	———
6	La Canche	10 16 23·6	
	Wrotham	10 21 18·9	
7	La Canche	10 26 25·0	
	Wrotham	10 31 15·3	
8	La Canche	———	
	Wrotham	10 41 18·5	
9	La Canche	10 46 27·8	
	Wrotham	10 51 16·4	
10	La Canche	10 56 20·0	
	Wrotham	11 1 17·9	

15 Juillet.

No.		h. m. s.	Remarques.
1	La Canche	———	
	Wrotham	9 31 22·4	
2	La Canche	———	
	Wrotham	9 41 23·0	
3	La Canche	9 46 24·8	
	Wrotham	———	
4	La Canche	———	
	Wrotham	———	
5	La Canche	———	
	Wrotham	10 11 23·4	———
6	La Canche	———	
	Wrotham	10 21 22·5	
7	La Canche	———	
	Wrotham	10 31 25·4	
8	La Canche	10 36 25·3	
	Wrotham	10 41 23·3	
9	La Canche	10 46 25·3	
	Wrotham	10 51 23·2	
10	La Canche	———	
	Wrotham	———	

16 Juillet.

No.		h. m. s.	Remarques.
1	La Canche	———	
	Wrotham	9 31 31·3	
2	La Canche	———	
	Wrotham	9 41 29·9	
3	La Canche	———	
	Wrotham	9 51 25·8	
4	La Canche	———	
	Wrotham	10 1 31·5	
5	La Canche	———	
	Wrotham	10 11 30·7	———
6	La Canche	———	
	Wrotham	10 21 32·6	
7	La Canche	———	
	Wrotham	10 31 28·9	
8	La Canche	———	
	Wrotham	10 41 35·3	
9	La Canche	———	
	Wrotham	10 51 30·0	
10	La Canche	10 56 29·7	
	Wrotham	11 1 29·8	

17 Juillet.

No.		h. m. s.	Remarques.
1	La Canche	9 26 20·8	
	Wrotham	9 31 36·0	
2	La Canche	9 36 20·3	Faible.
	Wrotham	9 41 35·9	
3	La Canche	———	
	Wrotham	9 51 37·0	
4	La Canche	9 56 27·7	
	Wrotham	10 1 37·4	
5	La Canche	10 6 27·9	Faible.
	Wrotham	10 11 35·6	
6	La Canche	———	
	Wrotham	10 21 38·8	
7	La Canche	10 26 27·7	
	Wrotham	10 31 40·7	
8	La Canche	10 36 24·8	Faible.
	Wrotham	10 41 40·8	
9	La Canche	10 46 38·5	Faible.
	Wrotham	10 51 32·0	
10	La Canche	———	
	Wrotham	11 1 38·4	

M. LARGETEAU'S *Observations at Fairlight continued.*

18 Juillet.			
No.		h. m. s.	Remarques.
1	La Canche	———	
	Wrotham	———	
2	La Canche	9 36 31·0	
	Wrotham	9 41 46·4	
3	La Canche	9 46 29·8	Faible.
	Wrotham	9 51 49·7	
4	La Canche	9 56 32·5	
	Wrotham	———	
5	La Canche	———	
	Wrotham	———	
6	La Canche	10 16 32·2	
	Wrotham	10 21 46·8	
	La Canche	10 26 24·5	Faible, Observation douteuse
7	Wrotham	10 31 42·5	Observation douteuse
8	La Canche	———	
	Wrotham	10 41 47·2	
	La Canche	10 46 30·3	Observation douteuse
9	Wrotham	10 51 43·0	
10	La Canche	10 56 25·0	
	Wrotham	11 1 42·4	

19 Juillet.			
No.		h. m. s.	Remarques.
1	La Canche	———	
	Wrotham	9 31 50·5	Observation douteuse
2	La Canche	9 36 33.2	
	Wrotham	9 42 0·4	
3	La Canche	9 46 33·5	
	Wrotham	9 51 53·5	
4	La Canche	9 56 28·8	
	Wrotham	10 1 56·6	
5	La Canche	———	
	Wrotham	10 11 50·5	Observation douteuse
6	La Canche	10 16 32·5	
	Wrotham	10 22 2·4	
7	La Canche	10 26 33·7	
	Wrotham	10 32 24·7	ou 14.7."
8	La Canche	10 36 35·1	Extremement faible.
	Wrotham	10 41 59·9	
9	La Canche	10 46 37·5	
	Wrotham	10 51 59·4	
10	La Canche	———	
	Wrotham	11 2 3·3	

20 Juillet.			
No.		h. m. s.	Remarques.
1	La Canche	———	
	Wrotham	9 31 43·5	
2	La Canche	———	
	Wrotham	9 41 58·2	
3	La Canche	———	
	Wrotham	9 51 56·4	
4	La Canche	———	
	Wrotham	10 1 56·5	
5	La Canche	———	
	Wrotham	10 11 57·8	
6	La Canche	———	
	Wrotham	10 22 4·5	
7	La Canche	———	
	Wrotham	10 31 57·7	
8	La Canche	———	
	Wrotham	10 41 58·3	incertaine.
9	La Canche	———	
	Wrotham	10 51 57·9	incertaine.
10	La Canche	———	
	Wrotham	———	

21 Juillet.			
No.		h. m. s.	Remarques.
1	La Canche	———	
	Wrotham	———	
	La Canche	9 36 36·0	
	Wrotham	9 42 7·6	
3	La Canche	9 46 38·9	Faible.
	Wrotham	———	
4	La Canche	———	
	Wrotham	———	
5	La Canche	10 6 39·8	
	Wrotham	10 12 0·4	1ère Explosion.
		———	2e
6	La Canche	———	
	Wrotham	———	
7	La Canche	10 26 38·2	
	Wrotham	10 32 1·9	1ère Explosion.
		8 5	2e
8	La Canche	10 36 47·9	
	Wrotham	10 42 0·9	1ère Explosion.
		———	2e
9	La Canche	10 46 41·8	
	Wrotham	10 52 2·3	1ère Explosion
		21·5	2e
10	La Canche	———	
	Wrotham	10 2 1·1	

M. LARGETEAU'S *observations at Fairlight continued.*

22 Juillet.

No.		h. m. s.	Remarques.
1	La Canche	9 26 35·3	
	Wrotham	9 32 8·9	
2	La Canche	9 36 39·8	
	Wrotham	9 42 6·9	
3	La Canche	9 46 42·4	
	Wrotham	9 52 8·5	
4	La Canche		
	Wrotham	10 2 9·7	
5	La Canche	10 6 57·7	
	Wrotham	10 12 0·7	Douteuse
6	La Canche	10 16 37·7	
	Wrotham	10 22 1·8	1ere Explosion }
		8·0	2e } Obs. incertaine.
			3e
7	La Canche	10 26 47·9	
	Wrotham	10 32 2·7	1ere Explosion } Obs. incertaine.
		9·1	2e }
8	La Canche	10 36 59·0	Douteuse.
	Wrotham		
9	La Canche	10 46 37·4	
	Wrotham		
10	La Canche	10 56 37·8	Très douteuse.
	Wrotham	11 2 2·7	

VI. *Observations made at the top of the Royal Observatory, Greenwich, on the rockets at Wrotham.*

July 11, 1825. The blue light and all the rockets were this evening distinctly seen by the naked eye. The observations were made with telescopes, by three observers, with the same chronometer. The chronometer was compared with the transit clock both before and after observation. The blue light appeared about $9^h\ 21^m\ 25^s$.

Rockets.	Appt Time I.	II.	III.	Mean *
1	$9^h\ 31^m\ 54{\cdot}4^s$	– – 54·3s	– – 54·2s	54·30s
2	9 41 49.25	– – 49·4	– – 49·2	49.28
3	9 51 45.75	– – 46.2	– – 45.8	45.92
4	10 1 47.5	– – 47.8	– – 47·4	47.56
5		– – 45.8	– – 45·4	45.60
6	10 21 46.75	– – 47.1	Absent.	46.92
7	10 31 46.8	– – 47.2	– – 47·3	47.10
8		– – 49·0	– – 48.4	48.70
9	10 51 45.8	– – 46.3	– – 46.2	46.10
10	11 1 50.4	– – 50.5	– – 50.6	50.50

	Chronometer	Clock.
Comparison before – –	$8^h\ 55^m$	$16^h\ 12^m\ 22^s{\cdot}06$
after – –	11 14	18 31 44·67

** Mean Error and Rate of Sidereal Clock.

Mean of transits of 5 * *	Corresponding mean error.	Rate.
$16^h\ 24^m$	48.36s	— 0.02

Chronometer fast $1^m\ 17.97^s$.

The loss of the fifth observation in column 1. was occasioned by some accidental derangement of the telescope. The loss of the eighth was occasioned by the rocket passing through the field of view before explosion. Observations 9 and 10, in column 1, were made with the naked eye.

* In taking the mean of the three observations, those marked (::) doubtful, are not considered.

** The transit observations employed throughout are reduced by the same system of corrections, and mean right ascensions, as those used at the observatory of Paris for that purpose; so that no error in the results, from a difference of catalogues or corrections, is introduced.

July 12. All the signals, the blue light excepted, were this evening visible to the naked eye; the blue light could not be seen at all:* the times of the explosions were this evening *all* observed with telescopes.

Rockets.	Appt Time. I.	II.		Mean.
1	9h 31 50s	9h 31m 49.8s	This evening no third observer.	49.90s
2	41 50.75	41 50.6		50.67
3	51 50.0	51 50.0		50.00
4	10 1 51.5	10 1 51.5		51.50
5	11 51.8	11 52.2		52.00
6	21 53.0	21 52.8		52.90
7	31 51.0	31 51.0		51.00
8	41 51.8	41 51.3		51.25
9	51 48.0	51 48.0		48.00
10	11 1 49.8	11 1 49.7		49.75

	Chronometer.	Clock.
Comparison before -	9h 11m	16h 32m 19.94s
After -	11 12	18 33 39.80

Mean Error and Rate of Transit Clock.

Mean of 5 *	Corresponding error.	Rate.
16h 24m	48.27s	— 0.08

From mean comparison on 11th, to ditto on 12th, chronometer gained 1.02s.

Chronometer fast 1m 18.99s.

Rockets 1 and 6 exploded twice, at an interval of about three seconds. The first explosion, in each case, was the one observed; the second, not being expected, was lost.

* None was fired. (H.)

July 13th. All the signals were visible to the naked eye.

Rockets.	App^{t} Time I.	II.	III.	Mean.
1	$9^h\ 31^m\ 55.8^s$	$9^h\ 31^m\ 55.6^s$	55.6	55.67
2	——	——	——	—
3	9 51 55.75	51 56.0	55.2	55.87
4	10 1 55.0	10 1 55.2	54.8	55.0
5	11 57.4	11 57.3	57.6	57.43
6	21 57.2	21 57.0	57.0	57.07
7	31 56.6	31 56.6	56.5	56.57
8	41 56.0	41 56.0	55.8	55.93
9	51 55.8	51 55.6	55.3	55.57
10	- - -	11 1 56.2	56.2	56.2

Chronometer.

Comparison.			
Before - -	$9^h\ 16^m$	$16^h\ 41^m\ 16.63^s$
After - -	11 13	18 38 35.75

Mean Error and Rate of Sidereal Clock.

Mean of 6 *'s.	Mean error corresponding.	Mean rate.
$16^h\ 40^m$	48.39^s	+ 0.14

Comparison 12th to ditto 13th. Chronometer, + 0.87.

Chronometer fast $1^m\ 19.86^s$.

The 2d rocket was lost by all the observers: it did not appear till some seconds after the time specified; and when it did appear it exploded immediately. It exploded *about* $9^h\ 42^m\ 22^s$.

The 10th rocket in column I. was lost by a derangement of the telescope.

The third observation, column III. is doubtful to half a second.

July 14.

Rockets.	App^{t} Time I.	- - II.	- - III.	Mean.
1	$9^h\ 31^m 59.0^s$	– – 59.2^s	- -	59.10^s
2	42 0.7	– – 0.4	- -	0.55
3	51 59.0	– – 59.0	- -	59.00
4	::10 1 58.8	– – 59.4	- -	59.40
5	12 3.4	– – 3.3	- -	3.35
6	22 3.2	– – 3.1	- -	3.15
7	31 59.4	– – 59.2	- -	59.30
8	42 2.4	– – 2.3	- -	2.35
9	52 0.3	– – 0.5	- -	0.5
10	11 2 1.8	– – 2.0	- -	1.9

Absent.

Comparison of Chronometer and Clock.

Chronometer	Clock.
Before - - $9^h\ 11^m$	$16^h\ 40^m\ 11.87^s$
After - - 11 15	18 44 32.13

Mean Error, and Rate of Sidereal Clock.

Mean of 7 ^{s}	Mean error corresponding.	Mean rate.
$16^h\ 21^m$	48.25^s	— 0.16

Comparison 13th to ditto 14th + 0.38.
Chronometer fast $1^m\ 20.24^s$.

July 15th. The third, fourth, and last rockets disappeared without any explosion. In the third column something like an explosion was noted at the beginning of the ascent of the third rocket, but no dependance can be placed on it.

Rockets.	App^{t} Time. I.	- - II.	- - III.	Mean.
1	$9^h\ 32^m\ 8.6^s$	– – 8.5^s	– – 8.4^s	8.5^s
2	42 8.8	– – 9.0	– – 9.0	8.93
3	———	———	———	—
4	———	———	::10 2 3.0	—
5	10 12 9.4	– – 9.0	– – 9.2	9.20
6	22 8.1	– – 8.0	– – 8.0	8.03
7	32 10.9	– – 11.2	– – 11.5	11.2
8	42 9.3	– – 9.2	– – 9.2	9.23
9	52 9.2	– – 9.0	– – 9.0	9.07
10	———	———	———	—

Comparison of Chronometer and Clock.

Chronometer.	Clock.
Before - - $9^h\ 24^m$	$16^h\ 57^m\ 9.80^s$
After - - 11 10	18 43 27.04

Mean Error, and Rate of Sidereal Clock.

Mean of 6 ^{s}	Mean error corresponding.	Mean rate.
$16^h\ 0^m$	47.92^s	— 0.30

Comparison 14th to ditto 15th + 0.47
Chronometer fast $1^m\ 20.71^s$.

July 16. The third rocket disappeared without explosion.

Rockets.	App^{t} Time. I.	- - II.	- - III.	Mean.
1	$9^{h}\,32^{m}\,17.0^{s}$	– – 16.9^{s}	– – 16.9^{s}	16.93^{s}
2	42 15.6	– – 15.6	– – 15.4	15.53
3	——	——	——	—
4	10 2 16.8	– – 17.2	– – 17.1	17.03
5	12 16.4	– – 16.3	– – 16.5	16.40
6	22 18.0	– – 17.9	– – 18.3	18.07
7	32 14.9	– – 14.8	– – 15.1	14.93
8	42 21.0	– – 21.0	– – 21.0	21.0
9	52 15.8	– – 15.2	– – 15.4	15.47
10	11 2 15.2	– – 15.1	– – 15.5	15.27

Comparison of Chronometer and Clock.

Chronometer.	Clock.
Before $9^{h}\,18^{m}$	$16^{h}\,55^{m}\,5.29^{s}$
After 11 12	18 49 24.06

Mean Error and Rate of Sidereal Clock.

Mean of 5 $*^{s}$	Mean error corresponding.	Mean rate.
16.6	47.60^{s}	— 0.33^{s}

Comparison from 15th to 16th — 0.31.

Chronometer fast $1^{m}\,20.40^{s}$.

July 17th. The loss of observation 1, in columns I. and III. was occasioned by the observers mistaking the minute. It was however very accurately taken by the second observer.

Rockets.	App^{a} Time I.	- - II.	- - III.	Mean.
1	——	$9^h\ 32^m\ 20.1^s$	——	20.1^s
2	$9^h\ 42^m\ 20.0^s$	42 19.9	:: $-^h\ -^m\ 19.1^s$	19.95
3	52 20.8	- - 21.3	- - 21.3	21.13
4	10 2 21.4	- - 21.4	- - 21.4	21.40
5	12 19.8	- - 19.3	- - 19.2	19.43
6	22 22.9	- - 22.8	- - 22.9	22.87
7	32 24.8	- - 24.8	- - 24.8	24.8
8	42 25.0	- - 25.0	- - 25.0	25.0
9	- - -	- - -	:: 10 52 16.0	—
10	11 2 22.5	- - 22.6	- - 22.6	22.57

Comparison of Chronometer and Clock.

Chronometer.	Clock.
Before $9^h\ 21^m$	$17^h\ 2^m\ 3.68^s$
After 11 11	18 52 21.72

Mean Error and Rate of Sidereal Clock.

Mean of 7 *s	Mean error.	Mean rate.
$15^h\ 30^m$	47.56^s	—0.06

Comparison from 16th to 17th — 1.40.
Chronometer fast $1^m\ 19.00^s$.

The ninth rocket exploded the moment it began to ascend: the time noted cannot be depended on.

July 18th. The first, seventh, ninth, and tenth rockets could not be observed; one or two exploded without ascending; the remainder did not explode at all.

Rockets.	App^{t} Time I.	- - II.	- - III.	Mean.
1	——	——	——	—
2	$9^{h}\,42^{m}\,29.1^{s}$	– – 29.0^{s}	– – 29.1^{s}	29.07^{s}
3	– 52 32.1	– – 32.3	– – 32.2	32.20
4	10 2 33.3	– – 33.0	– – 33.2	33.17
5	– 12 31.1	– – 31.0	– – 31.6	31.23
6	– 22 29.8	– – 29.6	– – 29.7	29.70
7	——	——	——	—
8	– 42 29.9	– – 30.0	– – 29.9	29.93
9	——	——	——	—
10	——	——	——	—

Comparison of Chronometer and Clock.

Chronometer.	Clock.
Before $9^{h}\,37^{m}$	$17^{h}\,22^{m}\,3.55^{s}$
After 11 11	18 56 18.95

Mean Error and Rate of Sidereal Clock.

Mean of 6 Θ^{s}	Mean error.	Mean rate.
$16^{h}\,0^{m}$	47.30^{s}	— 0.26^{s}

Comparison from the 17th to 18th — 0.93.
Chronometer fast $1^{m}\,18.07^{s}$.

July 19th. The fifth rocket could not be observed.

Rockets.	App^{t} Time I.	- - II.	- - III.	Mean.
1	$9^{h}\ 32^{m}\ 43.1^{s}$	- - 43.0^{s}	- - 43.0^{s}	43.03^{s}
2	- 42 41.0	- - 40.9	- - 41.0	40.97
3	- 52 34.1	- - 34.1	- - 34.2	34.13
4	10 2 36.9	- - 36.9	- - 36.9	36.9
5	——	——	——	—
6	- 22 42.9	- - 42.9	- - 42.9	42.9
7	- 33 4.8	——	- - 4.7	4.75
8	- 42 40.1	- - 40.0	- - 40.2	40.1
9	- 52 40.0	- - 40.0	- - 39.9	39.97
10	::11 2 43.3	:: - 43.1	- - 43.7	43.7

Comparison of Chronometer and Clock.

Chronometer.	Clock.
Before $8^{h}\ 48^{m}$	$16^{h}\ 36^{m}\ 53.84^{s}$
After 11 10	18 59 17.02

Mean Error and Rate of Sidereal Clock.

Mean of 3 *'s	Mean error.	Mean rate.
$16^{h}\ 50^{m}$	47.16^{s}	— 0.19^{s}

Comparison from 18th to 19th — 1.81

Chronometer fast 1^{m} 16.26^{s}

Observation 10 in columns 1 and 2 doubtful to half a second.

July 20. The rockets this evening were miserably bad; five only were observed; the eighth however *might* have been a good one; it was lost by all the observers looking for it too late.

Rockets.	Appt Time I.	- - - II.	- - - III.	Mean.
1	9h 32m 36.6s	36.7s	36.9s	36.73s
2	—	—	—	—
3	52 56.2	- - 56.0	- - 56.1	56.1
4	—	—	—	—
5	—	—	10 12 39.2	39.2
6	10 22 45.7	- - 45.8	- 22 46.1	45.87
7	—	10 32 53.0	- - 53.1	53.05
8	—	—	—	—
9	—	—	—	—
10	—	—	—	—

Comparisons of Chronometer and Clock.

Chronometer.	Clock.
Before 9h 6m	16h 58m 53.06s
After 11 10	19 3 13.34

Mean Error and Rate of Sidereal Clock.

Mean of 5 **	Mean error.	Mean rate
15h 40m	47.22s	— 0.03s

Comparison from 19th to 20th, + 0.35

Chronometer fast 1m 16.61s

July 21. The rockets much worse this evening than they were last. Only *one* out of the whole number mounted at all. All the others were seen, but nothing was sufficiently definite to admit of being noted. *

Rockets.	App^{t} Time I.	- - - II.	- - - III.	Mean.
1	—	—	—	—
2	$9^{h}\ 42^{m}\ 48.8^{s}$	- - 48.8^{s}	- - 48.8^{s}	48.8^{s}
3	—	—	—	—
4	—	—	—	—
5	—	—	—	—
6	—	—	—	—
7	—	—	—	—
8	—	—	—	—
9	—	—	—	—
10	—	—	—	—

Comparisons of Chronometer and Clock.

Chronometer.	Clock.
Before $9^{h}\ 13^{m}$	$1^{h}\ 9^{m}\ 50.88^{s}$
After 11 10	19 7 $9.9\overline{3}$

Mean Error and Rate of Sidereal Clock.

1 ^{m}.	Error.	Rate.
$17^{h}\ 26^{m}$.	47.37	+ 0.20^{s}

Comparisons from 20 to 21, + 1.02

Chronometer fast $1^{m}\ 17.63^{s}$

* It is much to be regretted that some attempt at least to note them was not made. Had it been done, this night's result, which is now dependent on a single signal, might perhaps (as they were for the most part tolerably well observed at Wrotham), have been placed nearly on the same footing with the rest. H.)

July 22. Rockets extremely bad; four only could be observed.

Rockets.	App^{t} Time I.	- - - II.	- - - III.	Mean.
1	$9^{h}\ 32^{m}\ 51.2^{s}$	51.35^{s}	51.4^{s}	51.32^{s}
2	− 42 49.9	− − 49.8	− − 49.6	49.77
3	− 52 51.4	− − 51.3	− − 51.3	51.33
4	——	10 2 52.4	− − 52.4	52.4
5	——	——	——	—
6	——	——	——	—
7	——	——	——	—
8	——	——	——	—
9	——	——	——	—
10	——	——	——	—

Comparisons of Chronometer and Clock.

Chronometer.	Clock.
Before $9^{h}\ 23^{m}$	$17^{h}\ 23^{m}\ 48^{s}$
After 11 11	19 12 5.59

Mean Error and Rate of Sidereal Clock.

Mean of 5^{ms}.	Mean error.	Mean rate.
$15^{h}\ 47^{m}$	47.57^{s}	+ 0.21

Comparisons from 21 to 22, + 0.32.

Chronometer fast $1^{m}\ 17.95^{s}$

The means of the Comparisons, with the true Sidereal Time corresponding.

	Chron.	Clock.	True Sidereal Time.
July 11	$10^{h}\ 4^{m}\ 30^{s}$	$17^{h}\ 22^{m}\ 3.36^{s}$	$17^{h}\ 21^{m}\ 15.0^{s}$
12	10 11 30	17 32 59.87	17 32 11.60
13	10 14 30	17 39 56.19	17 39 7.80
14	10 15 0	17 42 22.00	17 41 33.75
15	10 17 0	17 50 18.42	17 49 30.52
16	10 15 0	17 52 14.67	17 51 27.09
17	10 16 0	17 57 12.70	17 56 25.14
18	10 24 0	18 9 11.25	18 8 23.97
19	9 59 0	17 48 5.43	17 47 18.27
20	10 8 0	18 1 3.20	18 0 15.98
21	10 11 30	18 8 30.405	18 7 42.03
22	10 17 0	18 17 56.79	18 17 9.20

True Sidereal Time of the explosions.

July 11. Rockets.	True Time.	July 12. Rockets.	True Time.	July 13. Rockets.	True Time.
1	16h 48m 3.392s	1	16h 52m 24.98s	1	16h 56m 26.45s
2	— 58 30.54	2	17 2 27.38	2	———
3	17 8 28.82	3	— 12 28.27	3	17 16 29.95
4	— 18 32.11	4	— 22 31.51	4	— 26 30.73
5	— 28 31.79	5	— 32 33.66	5	— 36 34.81
6	— 38 34.85	6	— 42 36.21	6	— 46 36.09
7	— 48 36.56	7	— 52 35.93	7	— 56 37.33
8	— 58 39.80	8	18 2 37.82	8	18 6 38.23
9	18 8 38.84	9	— 12 36.19	9	— 16 39.50
10	— 18 44.8	10	— 22 39.59	10	— 26 41.77

July 14. Rockets.	True Time.	July 15 Rockets.	True Time.	July 16. Rockets.	True Time.
1	17h 0m 26.10s	1	17h 4m 31.64s	1	17h 8m 37.06s
2	— 10 29.20	2	— 14 33.72	2	— 18 37.34
3	— 20 29.30	3	———	3	———
4	— 30 31.34	4	———	4	— 38 42.03
5	— 40 36.94	5	— 44 39.04	5	— 48 43.04
6	— 50 38.39	6	— 54 39.39	6	— 58 46.36
7	18 0 36.00	7	18 4 44.21	7	18 8 44.95
8	— 10 40.87	8	— 14 43.88	8	— 18 52.58
9	— 20 40.66	9	— 24 45.35	9	— 28 48.68
10	— 30 43.71	10	———	10	— 38 50.12

July 17. Rockets.	True Time.	July 18. Rockets.	True Time.	July 19. Rockets.	True Time.
1	17h 12m 38.09s	1	———	1	17h 20m 56.91s
2	— 22 39.58	2	17h 26m 46.25s	2	— 30 56.55
3	— 32 44.40	3	— 36 51.02	3	— 40 51.34
4	— 42 44.30	4	— 46 53.62	4	— 50 55.77
5	— 52 43.96	5	— 56 53.31	5	———
6	18 2 49.06	6	18 6 53.42	6	18 11 5.09
7	— 12 52.63	7	———	7	— 21 28.65
8	— 22 52.48	8	18 26 57.05	8	— 31 5.58
9	———	9	———	9	— 41 7.11
10	— 42 55.29	10	———	10	— 51 12.50

July 20. Rockets.	True Time.	July 21. Rockets.	True Time.	July 22. Rockets.	True Time.
1	17h 24m 46.90s	1	———	1	17h 32m 53.27s
2	———	2	17h 38m 56.10s	2	— 42 53.36
3	17 45 9.60	3	———	3	— 52 56.56
4	———	4	———	4	18 2 59.28
5	18 4 55.95	5	———	5	———
6	— 15 4.28	6	———	6	———
7	— 25 13.12	7	———	7	———
8	———	8	———	8	———
9	———	9	———	9	———
10	———	10	———	10	———

Statement of the method of combining and calculating the Observations, and obtaining the Rates of the chronometers.

PREVIOUS to stating the result of these observations, it will not be irrelevant to explain the method pursued in reducing them, and the principles on which the calculation has been made; and it may be here remarked, that the brevity and facility of the computations which will appear to be required for this purpose, is not the least recommendation of the method itself.

Suppose A and Z to be the two extreme points whose difference of longitudes is to be determined, and at each of which the true sidereal time is supposed to be known by transits of well determined stars and registered by exact clocks, or carefully compared chronometers. Intermediate between these, suppose two, or any number of stations, B, C, &c. chosen, at each of which are placed observers furnished with telescopes and good chronometers; and again, intermediate between these, and in the order

A, *a*, B, *b*, C, *c*, Z,

let posts or stations *a*, *b*, *c*, be selected, at which signals are made, by the explosion of gunpowder, the discharge of rockets, the extinction of lamps, or otherwise, at regular concerted times, and so arranged that the signals at *a* shall be visible from both A and B; those at *b* from both B and C; and those at *c* from B and Z. Now let a signal be made at *a*, and observed both from A and B, and the moment of its happening noted at A by the sidereal clock, and at B by the

chronometer; then, if the observations were perfect, the difference of the clock at A, and the chronometer at B, would become exactly known. Let this be denoted by A — B. A short time after, let a signal be made at *b*, and observed by the chronometers at B and C, whose difference (which we will in like manner denote by B — C,) becomes thus precisely known at the time of making the signal. In the same manner may the difference C — Z of the chronometer at C and the sidereal clock at Z be known at the moment of explosion of a signal at *c*; and so on, if there be more intermediate stations.

Now, the clocks at A and Z being all along supposed to keep strict sidereal time, if the watches at B, C, did the same, it is manifest that the difference between any two of them determined at one moment would be the same at every other; and therefore the intervals elapsed between the signals would be out of the question, and the observations might all be regarded as simultaneous; so that the sum of the differences $(A-B)+(B-C)+(C-Z)=A-Z$ would express strictly the difference of the true sidereal times at the extreme points, *i. e.* their difference of longitudes expressed in time, without any further calculation or reduction.

It is equally evident that, whatever be the rates of the watches, if the intervals elapsed between the signals were infinitely small, so as to reduce their gain or loss in these times to nothing, the same would hold good. Since this however cannot be the case, it is obvious that the difference of longitudes so obtained will be affected by the rates of the watches and the intervals of the signals, which must accord-

ingly be allowed for. Now, as the intervals at which the signals are made at the successive stations are small (only five minutes), the gain or loss of the watches used may be calculated for such small times to great nicety; and, if the watches were regulated to sidereal time, and of any ordinary degree of goodness, the correction on this account would be almost insensible; or, if regulated, as is generally the case, to mean time, the reduction from mean to sidereal time only need be applied, neglecting the deviation of the rates from strict mean time. The calculation then becomes of extreme simplicity; for since the watches have equal rates, we have no occasion to apply any correction to their observed differences; and it will suffice to apply to the uncorrected value of Δ ($= A - Z$, or)

$$\Delta = (A - B) + (B' - C') + (C'' - Z'')$$

the mere reduction from mean to sidereal time for the interval elapsed between the first and last signal; or in other words (regarding the whole operation as a species of telegraphing), for the time the message has occupied in its transmission from one observatory to the other.*

For example. On the 19th, a signal was made at Mont Javoult, and noted at Paris to have happened at $18^h\ 39^m\ 52^s.5$ true sidereal time at Paris, and at Lignieres at $10^h\ 49^m\ 41^s.0$ by the Lignieres Chronometer. About 5^m after this, a signal made at La Canche was observed at Lignieres to happen at $10^h\ 54^m\ 58^s.2$, and at Fairlight at $10^h\ 46^m\ 37^s.5$ by the Fairlight chronometer. Finally, a third signal was made about 5^m later still at Wrotham, and observed at $10^h\ 51^m\ 59^s.4$ by

* Might not telegraphs be employed to ascertain the difference of longitudes of the stations between which they are established?

the Fairlight chronometer, and at $18^h\ 41^m\ 7^s.11$ true sidereal time at Greenwich. The calculation then stands thus

$+A =$	$+18$	39	$52^s.50$	$-B =$	-10	49	$41^s.00$	$B' - B =$	$+0$	5	$12^s.20$
$+B' =$	$+10$	54	$53^s.20$	$-C' =$	-10	46	$37^s.50$	$C'' - C' =$	$+0$	5	$21^s.90$
$+C' =$	$+10$	51	$59^s.40$	$-Z'' =$	-18	41	$7^s.11$	Sum	0	10	$34^s.10$
Sum $=$	$+38^h$	144^m	$165^s.10$		-38^h	136^m	$85^s.61$				

$= 0^h\ 8^m\ 79^s.49$ or $= 0^h\ 9^m\ 19^s.49$ the uncorrected value of Δ

Reduction from mean to Sid. T. for an interval of $10^m\ 34^s.10$ . . $= + 1^s.73$

$0\ 9\ 21^s.22 = \Delta$

the corrected difference of longitudes.

Such is the result of the transmission of a single signal along the line, and such the whole calculation required to deduce it. It is chosen at random from among the observations, yet is probably entitled to at least as much confidence as any value hitherto previously obtained; a circumstance which sets the excellence of this method in a very strong light.

Such would be the process of calculation in the simplest state of the data, viz. when the signals are seen along the whole line without a failure, so that each message so transmitted arrives at its destination and gives a complete result. But this (in the present instance at least) has not been always, or generally the case. It has much more commonly happened that a signal made at one station (*a* for instance, has not been simultaneously observed, or not observed at all, at A and at B, while the other signals, at *b*, *c*, &c. have been regularly seen and registered. In every such case (of which endless combinations may occur) a link of the chain fails, and no result can be obtained from this series of observations taken singly. A very slight consideration will suffice to show that were we

to reject all such broken series, the observations of a whole night might easily be thrown away, though capable of affording a result quite as good as any other. Such a case actually occurs in the observations of the 18th, where no complete transmission of any one signal from end to end of the line took place, yet the mean result of that night's observations deviates less than two-tenths of a second from the result finally adopted as the truth.

The most advantageous way of employing such a broken series of observations as we have described is not at once obvious. It may depend on circumstances too nice for calculation, and which can be felt only by the observers themselves. The fairest however, and that which by employing all the observations according to one uniform rule leaves nothing to partiality, seems to me to be the following.

Let A be the time marked by the sidereal clock at the first extreme station A, then calling E the time marked by the same clock at any assumed arbitrary epoch, A—E will denote the sidereal time elapsed since that epoch. Call β the rate or sidereal time of the chronometer at the 2d station (B), β being supposed negative when the chronometer loses, (as for instance when it shows mean time). At the same moment that the clock at A marks A, let this chronometer mark B, then, since $\beta(A-E)$ is the quantity it has gained, since the epochs, $B-\beta(A-E)$ must be the time it would have indicated, if instead of gaining or losing, it had kept true sidereal time since the epoch. Consequently (the clock being supposed to have no rate) $A-\{B-\beta(A-E)\}$ or $A-B+\beta(A-E)$ will be the difference of the clock and chronometer *reduced*

to this epoch, i. e. the difference they would have indicated if instead of comparing them at the time A, they had been compared at the time E.

Every signal simultaneously observed at A and B, gives a direct comparison of the clock and chronometer; but it is only when thus reduced to a fixed epoch that these comparisons become comparable *inter se*; but when so reduced their mean may be taken, and is of course preferable to the result of any single comparison. Hence if we put

$$P = \textit{mean of all the } (A - B) + \beta \times \textit{mean of all the } (A - E)$$

P will express the difference of the clock and chronometer at the epoch more probably than any of the individual values derived from single observations.

It follows therefore that at any other sidereal time A′, the time indicated by the chronometer at B, (or B′) may be calculated from the expression

$$B' = (A' - P) + \beta\,(A' - E) \qquad (a)$$

more probably than it can be derived from any single actual observation. This equation gives

$$A' = \frac{B' + P + \beta E}{1 + \beta} = B' + P - \beta\,(P + B' - E)$$

neglecting squares and higher powers of β, whence the time by the clock at A becomes known at any instant in terms of that shown by the watch at B.

Now let a signal be made between B and C, and noted to happen at the moment marked B′ by the watch at B, and C′ by that at C. Let β and γ denote their respective rates on sidereal time; then since $B' - \beta\,(A' - E)$ and $C' - \gamma\,(A' - E)$ are the times they would have marked had they kept strict

sidereal time since the epoch, their difference *reduced* to the fixed epoch will be

$$(B'-C')-(\beta-\gamma)(A'-E)$$

in which, substituting for A its value above found, we get

$$(B'-C')-(\beta-\gamma)(P+B'-E)$$

neglecting powers and products of β and γ. Putting then $Q=$*mean of all the* $(B'-C')-(\beta-\gamma)-$*mean of all the* $(P+B'-E)$ we get the most probable value of the difference of the chronometers at the epoch which can be obtained from any number of such comparisons.

Finally, if we make a comparison at any time A″ (Paris Sid. T.) between the watch at C and the clock at z, and call their indications at that moment C″ and Z″, their apparent difference will be C″—Z″, and their difference reduced to the epoch will be

$$(C''-Z'')-\gamma(A''-E).$$

But Q being the most probable difference between the chronometers B and C at the epoch, and $(\beta-\gamma)$ the difference of their rates

$$Q+(\beta-\gamma)(A''-E)$$

will be their difference at any other moment A″; hence

$$B''-C''=Q+(\beta-\gamma)(A''-E).$$

But by the equation (a) since B″ and A″ are corresponding times, we have

$$B''=A''-P+\beta(A''-E).$$

Consequently substituting this for B″ we get

$$C''=A''-P-Q+\gamma(A''-E)$$

whence

$$A''=P+Q+C''-\gamma(A''-E)$$
$$=P+Q+C''-\gamma(P+Q+C''-E)$$

neglecting the square and higher powers of γ:

Consequently, still neglecting the same things we get

$$C''-Z''-\gamma\left\{P+Q+C''-E\right\}$$

for the difference of the timekeepers C and Z reduced to the epoch, and putting

$R=$ *mean of all the* $(C''-Z'')-\gamma$.*mean of all the* $(P+Q+C''-E)$

R will be their most probable difference reduced to the fixed epoch.

P, Q, and R, being thus obtained, we must obviously have for the correct difference of longitudes,

$$\Delta=P+Q+R.$$

Now, substituting for P, Q, R, their values, this gives

$$\begin{aligned}\Delta=&\textit{mean of}\,(A-B)+\textit{mean of}\,(B'-C')+\textit{mean of}\,(C''-Z'')\\&+\beta.\textit{mean of}\,(A-E)\\&+(\gamma-\beta).\textit{mean of}\,(P+B'-E)\\&-\gamma.\textit{mean of}\,(P+Q+C''-E)\end{aligned}$$

that is, reducing,

$$\begin{aligned}\Delta=&\textit{mean of}\,(A-B)+\textit{mean of}\,(B'-C')+\textit{mean of}\,(C''-Z'')\\&+\beta.\textit{mean of}\,A+(\gamma-\beta).\textit{mean of}\,B'-\gamma.\textit{mean of}\,C''\\&-P\beta-Q\gamma.\end{aligned}$$

This value of Δ is however susceptible of still further reduction by substituting for P and Q their values; which if done, and the powers and products of β and γ neglected, as has all along been done, we get

$$\begin{aligned}\Delta=&\textit{mean of}\,(A-B)+\textit{mean of}\,(B'-C')+\textit{mean of}\,(C''-Z'')\\&+\beta.\textit{mean of}\,A+(\gamma-\beta)\,\textit{mean of}\,B'-\gamma.\textit{mean of}\,C''\\&-\beta.\textit{mean of}\,(A-B)-\gamma.\textit{mean of}\,(B'-C')\end{aligned}$$

that is, finally (since the numbers of the observations of A and of B are necessarily equal, and therefore the mean of the values of A—B is equal to the mean of A — the mean

of B, and so for the rest) reducing and striking out all the terms which destroy each other.

$\Delta =$ *mean of* A $-$ *mean of* B $+$ *mean of* B′ $-$ *mean of* C′ $+$
$+$ *mean of* C″ $-$ *mean of* Z″
$+ \beta \{$ *mean of* B $-$ *mean of* B′ $\} + \gamma \{$ *mean of* C′ $-$ *mean of* C″ $\}$

or simply, denoting by A, B, A′, B′, &c. no longer the individual observed times (to which there will be no occasion again to refer) but the means of all those which have corresponding observations.

$$\Delta = A - B + B' - C' + C'' - Z''$$
$$+ \beta (B - B') + \gamma (C' - C''($$

This expression is, as it obviously ought to be, independent of the arbitrary epoch E, which may be assumed any number of hours or days before or after the observations.

The first line of this value of Δ may be regarded as an approximate one; the second as a correction depending on the rates of the watches; and it is clear that the several portions of which this correction consists are the respective gains of the chronometers on Sid. T. during the *mean* amounts of the delay of the message between the several stations, taking the expression in its algebraical sense, where a negative delay corresponds to an anticipation.

If all the signals succeeded, the coefficients of β and γ would be each $0^h\,5^m$, and the amount of the correction would be $(\beta + \gamma) \cdot \frac{5^m}{24^h} = \frac{\beta + \gamma}{288}$. It would therefore require no less a deviation of one of the chronometers from its assumed rate than 29^{sec} per diem, or of both of them $14\frac{1}{2}$, and the same way, to produce an uncertainty in the result to the amount of a tenth of a second; deviations incompatible with the

character of ordinary good watches, not to speak of chronometers.

The worst case that can happen is where the *first signal only* at *a* gives corresponding observations at the stations adjacent, the *last only* at *b*, the first again *only* at *c*, and so on. In this case the coefficients of β and γ would each equal the whole interval between the first and last signal at each post, or (in the present case) $1^h\,30^m$. The correction here would be

$$1\tfrac{1}{2} \times \frac{\beta+\gamma}{24} = \frac{\beta+\gamma}{16}$$

In this extreme case, the sum of the deviations of both watches from their assumed rates, need only amount to $1^s.6$ to produce an uncertainty of a tenth of a second in the result; and though such a case as here supposed is in the last degree improbable, yet as a certain approach to it is not unlikely, it may be of use to show how the rates of the watches, if not otherwise known, may be obtained, or if known, verified, by the observations themselves.

If we consider the observations on two successive nights, at two of the extreme stations, A and B for instance, calling A and B the means of the simultaneous observations on the first night, and $A_,$ $B_,$ on the second, we have, assuming for an epoch some time E = any number of days before either of the night's observations,

$$P = A - B + \beta(A - E)$$

But since this is generally true, if the observations be made in sufficient number on both nights to destroy their individual errors in the mean result, we must also have

$$P = A_, - B_, + \beta(A_, - E)$$

equating which we get

$$A - B - \beta(A - E) = A_{,} - B_{,} - \beta(A_{,} - E)$$

whence we find

$$-\beta = \frac{(A_{,} - B_{,}) - (A - B)}{A_{,} - A}$$

In this formula it is to be observed that $A_{,}$ and $B_{,}$ are each greater than 24 hours; but as timekeepers only register excesses above 12 hours and its multiples, if we wish to denote by $A_{,}$ and $B_{,}$ the mere readings off of the timekeepers, we must put $24^h + A_{,}$ and $24^h + B_{,}$ for $A_{,}$ and $B_{,}$ if the interval be one day; $48^h + A_{,}$ and $48^h + B_{,}$ if two days, and so on, so that (n being the number of days elapsed) we get

$$-\beta = \frac{(A_{,} - B_{,}) - (A - B)}{n \times 24^h + A_{,} - A}.$$

In like manner may the rate γ of the chronometer at C be found by comparison with the clock at Z thus,

$$-\gamma = \frac{(Z_{,}'' - C_{,}'') - (Z' - C')}{n \times 24^h + Z_{,}'' - Z'}.$$

If there be intermediate chronometers, the rate of each on that immediately preceding or following it may be found in exactly the same way.

Computation of the Rates of the Chronometers.

From the 18th to the 19th.

1. Lignieres Chronometer, or that at station B. Motel, No. 39.

19th.	$A_{,} - B_{,} = 7^h\ 50^m\ 7^s{\cdot}90$	$A_{,} = 18^h\ 19^m\ 41^s{\cdot}83$
18th.	$A - B = 7\ 46\ 8{\cdot}28$	$A = 18\ 32\ 21{\cdot}88$
	$(A_{,} - B_{,}) - (A - B) = +3\ 59{\cdot}62$	$A_{,} - A = -0\ 12^m\ 40^s{\cdot}05$

$$-\beta = \frac{3^m\ 59^s{\cdot}62}{24^h - 0^h\ 12^m\ 40^s{\cdot}05} = 4^m\ 1^s{\cdot}74\ ;\quad \beta = -\ 4^m\ 1^s{\cdot}74 \div 24^h$$

Whence the rate *on mean time* $= -5^s{\cdot}83$.

2. Fairlight Chronometer, at Station C. Baker, No. 744.

19th. $Z_1 - C_1 = 7^h\,49^m\,2^s{\cdot}75$ $\quad Z_1 = 18^h\,19^m\,20^s{\cdot}32$

$Z - C = 7\;45\;4{\cdot}31$ $\quad Z = 17\;53\;32{\cdot}44$

$+\,3\;58{\cdot}44$ $\quad +\,0\;18\;47{\cdot}88$

$$\beta = -\frac{3^m\,58^s{\cdot}44}{24^h\,18^m\,47^s{\cdot}88} = -\,3^m\,55^s{\cdot}36 \div 24^h$$

and rate on mean time $= +\,0^s{\cdot}55$.

Rates of the Chronometers from the 19th to the 21st.

For Motel, No. 39.

$A_1 - B_1 = 7^h\,58^m\,3^s{\cdot}69$ $\quad A_1 = 18^h\,14^m\,15^s{\cdot}18$

$A - B = 7\;50\;7{\cdot}90$ $\quad A = 18\;19\;41{\cdot}83$

$+\,7\;55{\cdot}79$ $\quad -\,5\;26{\cdot}65$

$$\beta = -\frac{7^m\,55^s{\cdot}79}{2 \times 24^h - 5^m\,26^s{\cdot}65} = -\,3^m\,58^s{\cdot}43 \div 24^h$$

being a rate of $-\,2^s{\cdot}52$ on mean time.

For Baker, No. 744.

$Z_1 - C_1 = 7^h\,56^m\,48^s{\cdot}40$ $\quad Z_1 = 17^h\,38^m\,56^s{\cdot}10$

$Z - C = 7\;49\;2{\cdot}75$ $\quad Z = 18\;12\;20{\cdot}32$

$+\,7\;45{\cdot}65$ $\quad -\,0\;33\;24{\cdot}22$

$$\beta = -\frac{7^m\,45^s{\cdot}65}{2 \times 24^h - 0^h\,33^m\,24^s{\cdot}22} = -\,3^m\,55^s{\cdot}56 \div 24^h$$

Being a rate of $+\,0^s{\cdot}35$ on mean time.

Rates of the Chronometers from the 21st to the 22d.

Motel, No. 39.

$A_1 - B_1 = 8^h\;2^m\;0^s{\cdot}14$ $\quad A_1 = 18^h\,11^m\,24^s{\cdot}77$

$A - B = 7\;58\;3{\cdot}69$ $\quad A = 18\;14\;15{\cdot}18$

$+\,0\;3\;56{\cdot}45$ $\quad -\,0\;2\;50{\cdot}41$

$$\beta = -\frac{3^m\,56^s{\cdot}45}{24^h - 2^m\,50^s{\cdot}41} = -\,3^m\,56^s{\cdot}92 \div 24^h$$

being a rate of $-\,1^s{\cdot}01$ on mean time.

Rates of the Chronometers from the 21st to the 22d.

Baker, No. 744.

$Z_{,} - C_{,} = 8^{h}\ 0^{m}\ 47^{s}\cdot04$	$Z_{,} = 17^{h}\ 47^{m}\ 55^{s}\cdot62$
$Z - C = 7\ 56\ 48\cdot40$	$Z = 17\ 38\ 56\cdot10$
$+\ 3\ 58\cdot64$	$+\ 0\ 8\ 59\cdot52$

$$\beta = -\frac{3^{m}\ 58^{s}\cdot64}{24^{h}\ 8^{m}\ 59^{s}\cdot52} = -\ 3^{m}\ 57^{s}\cdot16 \div 24^{h}$$

Being a rate of — 1″·25 on mean time.

The rates originally assigned to the chronometers on leaving Paris and London, were respectively (on mean time),

Motel No. 39, + 1″.8. Baker 744, + 1″.20.

The former, then, in the interval must have altered its rate (if that deduced from the observations of the 18th and 19th be correct), no less than — 7″·63; and between the 18th and 21st, must have again accelerated its daily rate by 3″·31, fluctuations not to be supposed in a chronometer of any character. It is therefore probable that the rate — 5″·83 of the 18th-19th is incorrect, and the observations being positive, and liable to no errors capable of accounting for so large a deviation, the cause, on this supposition, can lie nowhere but in some accidental derangement in that interval. Now it unfortunately happens, that the interval B — B′, on the 18th, to which this suspicious rate is to be applied, is no less than 41^{m} 20^{s}·6, which produces a correction of — 0″·17, or nearly two-tenths of a second in the result of that night's observations.

If we examine the individual observations of both nights, on which this rate depends, we shall find no satisfaction, though they tend to confirm the suspicion of a derangement

in the intervening day, by indicating rather a gain, than a loss on mean time;—but the unavoidable errors of observation will not permit the deduction of a rate from such short intervals as those elapsed during the observations of a single night.

However, we may be relieved from the disagreeable necessity of rejecting the night's observations on this score, by reflecting, that all observations are liable to *some* errors; that if we reject this on account of a suspected error of two tenths of a second, arising from the fault of a chronometer, we certainly should not be warrantable in retaining the result of the observations of the 21st, where the whole night's work rests on a single signal, and on the transit of a single star observed at Greenwich, and where an error to the extent of nearly half a second, from both causes united, may very fairly be presumed. We may be relieved, I say, from the necessity of rejecting observations where there are assuredly none to spare, by remarking that, according to any fair estimation of the weight of each night's result from the number of observations, the most suspicious result, that of the 21st, is precisely that which holds the lowest rank—and that whether we retain or reject those of the two nights in question, the ultimate result will (as will hereafter appear), be unaffected to the extent of more than three-hundredths of a second.

Actual Calculation and Results.

Computation of the observations of the 18th.

1st Combination. All the observers taken jointly.

A	B	B′	C′	C″	Z″
18h 15m 40″·37	10h 29m 34″·4	9h 54m 52″·0	9h 46m 29″·75	9h 41m 46″·2	17h 26m 46″·25
18 35 41 ·13	10 49 32 ·8	10 14 54 ·0	10 6 31 ·40	9 51 49 ·6	17 36 51 ·02
18 45 44 ·13	10 59 33 ·6			10 1 50 ·3	17 46 53 ·62
				10 11 48 ·6	17 56 53 ·31
				10 21 46 ·9	18 6 53 ·42
				10 41 47 ·2	18 26 57 ·05
Mean.	Mean.	Mean.	Mean.	Mean.	Mean.
18 32 21 ·88	10 46 13 · 6	10 4 53 ·0	9 56 30 ·57	10 8 28 ·13	17 53 32 ·44

A — B =	7 46 8 ·28	B — B′ =	+ 41 20 ·60	Gain on M. T. of B	— 0 ·17
B′ — C′ =	0 8 22 ·43	C′ — C″ =	— 11 57 ·56	—— of C	— 0 ·00
	7 54 30 ·71		+ 29 23 ·04		
C″ — Z″ =	— 7 45 4 ·31	Gain of mean on Sid. Time -	— 4 ·82		
	0 9 26 ·40				
	— 4 ·82				
	— 0 ·17				
	0 ·00				
	0 9 21 ·41	= Corrected difference of Longitudes.			

Computation of the observations of the 19th.

1st Combination. All the observers taken jointly.

A	B	B′	C′	C″	Z″
17h 29m 29″·6	9h 39m 30″·4	9h 44m 49″·4	9h 36 33″·10	9h 42m 0″·45	17h 30m 56″·55
18 39 52 ·5	10 49 41 ·1	9 54 49 ·9	9 46 36 ·65	9 51 53 ·65	17 40 51 ·34
18 49 43 ·4	10 59 30 ·4	10 34 49 ·7	10 26 33 ·70	10 1 56 ·50	17 50 55 ·77
		10 54 53 ·4	10 46 37 ·55	10 22 2 ·45	18 11 5 ·09
				10 32 24 ·75	18 21 28 ·65
				10 41 59 ·80	18 31 5 ·58
				10 51 59 ·60	18 41 7 ·11
				11 2 3 ·40	18 51 12 ·50
Mean.	Mean.	Mean.	Mean.	Mean.	Mean.
18 19 41 ·83	10 29 33 ·93	10 17 20 ·60	10 9 4 ·49	10 23 17 ·57	18 12 20 ·32

A — B =	+ 7 50 7 ·90	B — B′ =	+ 12 13 ·33	Gain of B on M. T.	— 0 ·02
B′ — C′ =	+ 8 16 ·11	C′ — C″ =	— 14 13 ·02	of C ——	0 ·00
	+ 7 58 24 ·01		— 1 59 ·69		
C″ — Z″ =	— 7 49 2 ·75	Gain of mean on Sid. T. - -	+ 0 ·33		
	9 21 ·26				
	+ 0 ·33				
	— 0 ·02				
	0 ·00				
	0 9 21 ·57	= Corrected difference of Longitudes.			

Calculation of the observations of the 21st.

1st. Combination. All the observers jointly.

A	B	B′	C′	C″	Z″
17h 37m 23s·10	9h 39m 24s·70	9h 54m 50s·40	9h 46m 38s·95	9h 42m 7s·7	17h 38m 56s·10
17 47 32·10	9 49 32·70	10 4 53·10	9 56 41·50		
18 7 40·95	10 9 38·60	10 14 51·20	10 6 39·80		
18 17 30·30	10 19 26·40	10 34 49·60	10 26 38·25		
18 37 40·75	10 39 33·20	10 44 59·40	10 36 47·90		
18 57 43·90	10 59 33·30	11 4 51·80	10 56 41·10		
Mean.	Mean.	Mean.	Mean.	Mean.	Mean.
18 14 15·18	10 16 11·49	10 26 32·58	10 18 21·25	9 42 7·7	17 38 56·10

A − B = 7 58 3·69 — B − B′ = −0 10 21·09 — Gain of B on M. T. = + 0·01

B′ − C′ = 8 11·33 — C′ − C″ = +0 36 13·55 — C . . . − 0·03

8 6 15·02 — +0 25 52·46

C′ − Z″ = −7 56 48·40 — Gain of mean on Sid. Time − { −4·54

9 26·62
−4·54
+0·01
−0·03

0 9 22.06 = Corrected difference of Longitudes.

Calculation of the observations of the 22d.

1st. Combination. All the observers jointly.

A	B	B′	C′	C″	Z″
17h 31m 12s·15	9h 29m 18s·6	9h 44m 50s·50	9h 36m 39s·80	9h 32m 8s·95	17h 32m 53s·27
17 51 18·70	9 49 21·9	9 54 53·50	9 46 42·45	9 42 6·95	17 42 53·36
18 1 15·65	9 59 17·1	10 4 53·20	9 56 42·50	9 52 8·55	17 52 56·56
18 11 21·70	10 9 21·7	10 15 8·60	10 6 57·85	10 2 9·90	18 2 59·28
18 21 43·60	10 19 41·7	10 24 48·30	10 16 37·50		
18 31 31·80	10 29 28·4	10 34 58·70	10 26 48·05		
18 51 29·80	10 49 23·0	10 44 57·60	10 36 47·00		
		10 54 47·70	10 46 37·15		
		11 4 48.80	10 56 37·90		
Mean.	Mean.	Mean.	Mean.	Mean.	Mean.
18 11 24·77	10 9 24·63	10 24 54·10	10 16 43·36	9 47 8·58	17 47 55·62

A − B = 8 2 0·14 — B − B′ = − 15 29·47 — Gain of B on M. T. + 0·01

B′ − C′ = 8 10·74 — C′ − C″ = + 29 34·78 — C . . . −0·03

8 10 10·88 — + 14 5·31

C″ − Z″ = −8 0 47·04 — Gain of Mean on Sid. Time − { − 2·31

9 23·84
−2·31
+0·01
−3·03

0 9 21·51 = Corrected difference of Longitudes.

Calculation of the observations of the 18th.

2d Combination. Capt. SABINE. Mr. HERSCHEL.

A	B	B′	C′	C″	Z″
$18^h\ 15^m\ 40^s{\cdot}37$	$10^h\ 29^m\ 34^s{\cdot}4$	$9^h\ 54^m\ 52^s$	$9^h\ 46^m\ 29^s{\cdot}7$	$9^h\ 41^m\ 46^s{\cdot}0$	$17^h\ 26^m\ 46^s{\cdot}25$
35 41·13	49 32·8	10 14 54	10 6 31·4	51 49·5	36 51·02
45 44·13	59 33·6			10 1 50·3	46 53·62
				11 48.6	56 53·31
				21 47.0	18 6 53·42
				41 47·2	26 57·05
Mean =	Mean =	Mean =	Mean =	Mean =	Mean =
18 32 21·88	10 46 13·6	10 4 53	9 56 30·55	10 8 28·1	17 53 32·44
A − B =	7 46 8·28		B − B′ = +	41 20·60	Gain on M.T.−0·17
B′ − C′ =	0 8 22·45		C′ − C″ = −	11 57·55	Gain on M.T.−0·01
	7 54 30·73	(B − B′) + (C′ − C″) =		29 23·05	
C″ − Z″ =	−7 45 4·34	Gain of mean on Sid. Time =	−4.82		
	0 9 26·39				
	−4·82				
	−0·17				
	−0·01				
	0 9 21·39 = The corrected difference of Longitudes.				

Calculation of the observations of the 19th.

2nd Combination. Observations of Capt. SABINE and Mr. HERSCHEL.

A	B	B′	C′	C″	Z″
$17^h\ 29^m\ 29^s{\cdot}6$	$9^h\ 39^m\ 30^s{\cdot}4$	$9^h\ 54^m\ 50^s{\cdot}0$	$9^h\ 46^m\ 33^s{\cdot}8$	$9^h\ 42^m\ 0^s{\cdot}5$	$17^h\ 30^m\ 56^s{\cdot}55$
18 39 52·5	10 49 41·2	10 34 49·6	10 26 33·7	9 51 53·8	17 40 51·34
18 49 43·4	10 59 30·0	10 54 53·6	10 46 37·6	10 1 56·4	17 50 55·77
				10 22 2·5	18 11 5·09
				10 32 24·8	18 21 28·65
				10 41 59·7	18 31 5·58
				10 51 59·8	18 41 7·11
				11 2 3·5	18 51 12·50
Mean.	Mean.	Mean.	Mean.	Mean.	Mean.
18 19 41·83	10 29 33·87	10 28 11·07	10 19 55·03	10 23 17·63	18 12 20·32
A − B =	+ 7 50 7·96	B − B′ = +	0 1 22·8	Gain of B on M.T. =	0·00
B′ − C′ =	+ 0 8 16·04	C′ − C″ = −	0 3 22·6	—— C ——	0·00
	+ 7 58 24·00		− 0 1 59·8		
C″ − Z″ =	− 7 49 2·69	Gain of mean on Sid. Time =	+0·33		
	9 21·31				
	+0·33				
	+0·00				
	−0·00				
	0 9 21·64	Corrected difference of Longitudes.			

Calculation of the observations of the 21st.

2d Combination. Observations of Capt. SABINE and Mr. HERSCHEL.

A	B	B′	C′	C′	Z′
$17^h\ 37^m\ 23^s{\cdot}10$	$9^h\ 39^m\ 24^s{\cdot}8$	$9^h\ 54^m\ 50^s{\cdot}4$	$9^h\ 46^m\ 39^s{\cdot}0$	$9^h\ 42^m\ 7^s{\cdot}7$	$17^h\ 38^m\ 56^s{\cdot}10$
17 47 32·10	9 49 32·8	10 4 53·2	9 56 41·5		
17 7 40·95	10 9 38·4	10 14 51·2	10 6 39·8		
18 17 30·30	10 19 26·4	10 34 49·6	10 26 38·3		
18 37 40·75	10 39 33·2	10 44 59·4	10 36 47·9		
18 57 43·90	10 59 33·2	11 4 52·0	10 56 41·1		
Mean.	Mean.	Mean.	Mean.	Mean.	Mean.
18 14 15·18	10 16 11·47	10 26 32·63	10 18 21·27	9 42 7·7	17 38 56·10
A — B =	7 58 3·71	B — B′ =	−0 10 21·16	Gain of B on mean T.	−0·01
B′ — C′ =	8 11·36	C′ — C″ =	+0 36 13·57	of C - -	+0·3
	8 6 15·07		+0 25 52·41		
C″ — Z′ =	−7 56 48·40	Gain of mean on Sid. T. -	−4·54		
	9 26·67				
	−4·54				
	−0·01				
	+0·03				
	0 9 22·15	Corrected difference of Longitudes.			

Calculation of the observations of the 22d.

2d Combination. Observations of Capt. SABINE and Mr. HERSCHEL.

A	B	B′	C′	C′	Z′
$17^h\ 31^m\ 12^s{\cdot}15$	$9^h\ 29^m\ 18^s{\cdot}6$	$9^h\ 44^m\ 50^s{\cdot}8$	$9^h\ 36^m\ 39^s{\cdot}8$	$9^h\ 32^m\ 9^s{\cdot}0$	$17^h\ 32^m\ 53^s{\cdot}27$
17 51 18·70	9 49 22·0	9 54 53·6	9 46 42·5	9 42 7·0	17 42 53·36
18 1 15·65	9 59 17·2	10 4 53·2	9 56 42·5	9 52 8·6	17 52 56·56
18 11 21·70	10 9 22·0	10 15 8·8	10 6 58·0	10 2 10·1	18 2 59·28
18 21 43·60	10 19 41·6	10 24 48·4	10 16 37·3		
18 31 31·80	10 29 28·6	10 34 58·8	10 26 48·2		
		10 44 57·6	10 36 47·0		
		10 54 48·0	10 46 36·9		
		11 4 48·8	10 56 38·0		
Mean.	Mean.	Mean.	Mean.	Mean.	Mean.
18 4 43·93	10 2 45·00	10 24 54·22	10 16 43·36	9 47 8·67	17 47 55·62
A — B =	8 1 58·93	B — B′ =	— 22 9·2	Gain of B on M. T.	−0·03
B′ — C′ =	0 8 10·86	C′ — C″ =	+ 29 34·7	of C - -	+0·03
	8 10 9·79		+ 7 25·5		
C″ — Z′ =	8 0 46·95	Gain on Sid. T.	−1·22		
	9 22·84				
	— 1·22				
	— 0·03				
	+ 0·03				
	0 9 21·62	= Corrected difference of Longitudes.			

Calculation of the observations of the 18th.

3d Combination. Capt. SABINE (for Col. BONNE)—M. LARGETEAU.

A	B	B′	C′	C″	Z″
18h 15m 40s·37	10h 29m 34s·4	9h 54m 52s·0	9h 46m 29s·8	9h 41m 46s·4	17h 26m 46s·25
35 41 ·13	49 32 ·8			51 49 ·7	36 51 ·02
45 44 ·13	59 33 ·6			10 21 46 ·8	18 6 53 ·42
				41 47 ·2	26 57 ·05
Mean.	Mean.	Mean.	Mean.	Mean.	Mean.
18 32 21 ·88	10 46 13 ·6	9 54 52 ·0	9 46 29 ·8	10 9 17 ·32	17 54 21 ·93
A — B =	7 46 8 ·28	B — B′ =	0 51 21 ·6	Gain of B on M. T. =	— 0 ·21
B′ — C′ =	0 8 22 ·20	C′ — C″ =	— 0 22 47 ·7	of C - -	— 0 ·02
	7 54 30 ·48		0 28 33 ·9		
C′ — Z″ =	—7 45 4 ·41	Gain of mean on Sid. T. - -	= — 4 ·68		
	0 9 26 ·07				
	— 4 ·68				
	— 0 ·21				
	— 0 ·02				
Δ =	0 9 21 ·16	The corrected difference of Longitudes.			

Calculation of the observations of the 19th.

3d Combination. Col. BONNE and M. LARGETEAU.

A	B	B′	C′	C″	Z″
18h 39m 52s·5	10h 49m 41s·0	9h 44m 49s·4	9h 36m 33s·2	9h 42m 0s·4	17h 30m 56s·55
18 49 43 ·4	10 59 30 ·8	9 54 49 ·8	9 46 33 ·5	9 51 53 ·5	17 40 51 ·34
		10 34 49 ·8	10 26 33 ·7	10 1 56 ·6	17 50 55 ·77
		10 54 53 ·2	10 46 37 ·5	10 22 2 ·4	18 11 5 ·09
				10 32 24 ·7	18 21 28 ·65
				10 41 59 ·9	18 31 5 ·58
				10 51 59 ·4	18 41 7 ·11
				11 2 3 ·3	18 51 12 ·50
Mean.	Mean.	Mean.	Mean.	Mean.	Mean.
18 44 47 ·95	10 54 35 ·90	10 17 20 ·55	10 9 4 ·47	10 23 17 ·52	18 12 20 ·32
A — B =	7 50 12 ·05	B — B′ =	+ 0 37 15 ·35	Gain of B on M. T. =	+ 0 ·04
B′ — C′ =	8 16 ·08	C′ — C″ =	— 0 14 13 ·05	C - -	— 0 ·01
	7 58 28 ·13		+ 0 23 2 ·3		
C′ — Z″ = —	7 49 2 ·80	Gain of mean on Sid. Time	= — 3 ·78		
	9 25 ·33				
	— 3 ·78				
	+ 0 ·04				
	— 0 ·01				
	0 9 21 ·58	Corrected difference of Longitudes.			

Calculation of the observations of the 21st.

3d Combination. Observations of Col. BONNE and M. LARGETEAU.

A	B	B′	C′	C″	Z″
17h 37m 23s·10	9h 39m 24s·6	10h 34m 49s·6	10h 26m 38s·2	9h 42m 7s·7	17h 38m 56s·10
17 47 32 ·10	9 49 32 ·6	10 44 59 ·4	10 36 47 ·9		
18 7 40 ·95	10 9 38 ·8				
18 57 43 ·90	10 59 33 ·4				
Mean.	Mean.	Mean.	Mean.	Mean.	Mean.
18 7 35 ·01	10 9 32 ·35	10 39 54 ·50	10 31 43 ·05	9 42 7 ·7	17 38 56 ·10
A — B =	7 58 2 ·66	B — B′ =	— 30 22 ·2	Gain of B on mean T. — 0 ·04	
B′ — C′ =	8 11 ·45	C′ — C″ =	+ 49 35 ·3	of C - - + 0 ·04	
	8 6 14 ·11		+ 19 13 ·1		
C″ — Z″ =	−7 56 48 ·40	Gain of mean on Sid. T. - -	— 3 ·15		
	9 25 ·71				
	−3 ·15				
	−0 ·04				
	+0 ·04				
	0 9 22 ·57	Corrected difference of Longitudes.			

Calculation of the observations of the 22d.

3d. Combination. Col. BONNE and M. LARGETEAU.

A	B	B′	C′	C″	Z″
17h 51m 18s·70	9h 49m 21s·8	9h 44m 50s·2	9h 36m 39s·8	9h 32m 8s·9	17h 32m 53s·27
18 1 15 ·65	9 59 17 ·0	9 54 53 ·4	9 46 42 ·4	9 42 6 ·9	17 42 53 ·36
18 11 21 ·70	10 9 21 ·4	10 15 8 ·4	10 6 57 ·	9 52 8 ·5	17 52 56 ·56
18 21 43 ·60	10 19 41 ·8	10 24 48 ·2	10 16 37 ·7	10 2 9 ·7	18 2 59 ·28
18 31 31 ·80	10 29 28 ·2	10 34 58 ·6	10 26 47 ·9		
18 51 29 ·80	10 49 23 ·0	10 54 47 ·4	10 46 37 ·4		
		11 4 48 ·8	10 56 37 ·8		
Mean.	Mean.	Mean.	Mean.	Mean.	Mean.
18 18 6 ·88	10 16 5 ·53	10 24 53 ·57	10 16 42 ·96	9 47 8 ·50	17 47 55 ·62
A — B =	8 2 1 ·35	B — B′ =	— 8 48 ·1	Gain of B on M. T. = −0 ·01	
B′ — C′ =	8 10 ·61	C′ — C″ =	+ 29 34 ·5	of C - - +0 ·02	
	8 10 11 ·96		+ 20 46 ·4		
C″ — Z″ =	−8 0 47 ·12	Gain of mean on Sid. T. - -	= −3 ·40		
	9 24 ·84				
	— 3 ·40				
	— 0 ·01				
	+ 0 ·02				
	0 9 21 ·45	= Corrected difference of Longitudes.			

In appreciating the weights to be attributed to these several results, it is obvious that the numbers of corresponding observations at each pair of stations, and of transits at the observatories, as it essentially influences the probable accuracy of the mean comparison of their timekeepers must be the elements of all fair estimations. If corresponding observations at any station be wanting, the weight is evidently nothing; so that calling x, y, z, the numbers of corresponding observations at A and B, at B and C, and at C and Z respectively, $x \times y \times z$ must necessarily be a multiplier of the function expressing the joint weight of the whole. But if the number of observations at any one station, or at all, be infinitely multiplied, the weight is clearly not infinite. If at *all* the stations, it would afford only such a degree of evidence as a perfect comparison of the clocks would give, which is but a relative certainty, after all, and may be denoted by unity. In like manner, if the observations at any one pair of stations be infinitely multiplied, the result is still open to all the errors of imperfect observations at the rest, so that unity will in like manner be the maximum of the coefficient depending on any separate set. The function

$$\frac{x}{x+1} \times \frac{y}{y+1} \times \frac{z}{z+1}$$

is the simplest which satisfies these conditions, each factor vanishing when its variable is o, and becoming unity when infinite. The same reasoning applies to the transit observations by which the clocks are compared with the stars, so that calling T and t the number of transit observations taken at each, by which the clock's errors are obtained, the function expressive of the weight of any night's observations will be

$$W = \frac{T}{T+1} \times \frac{x}{x+1} \times \frac{y}{y+1} \times \frac{z}{z+1} \times \frac{t}{t+1}.$$

It would be needless refinement to enquire minutely how far this agrees with a strict calculation of probabilities.

The result of the whole operation may then be briefly stated as follows:

Day of Obs.	Δ	x	y	z	T	t	W	W × (Δ − 9m 21s)
18th.	9m 21s·41	3	2	6	5	6	$\frac{3}{4}\cdot\frac{2}{3}\cdot\frac{6}{7}\cdot\frac{5}{6}\cdot\frac{6}{7}$ = 0·31	0·1271
19th.	9	21 ·57	3	4	8	3	3	$\frac{3}{4}\cdot\frac{4}{5}\cdot\frac{8}{9}\cdot\frac{3}{4}\cdot\frac{3}{4}$ = 0·30	0·1710
21st.	9	22 ·06	6	6	1	4	1	$\frac{6}{7}\cdot\frac{6}{7}\cdot\frac{1}{2}\cdot\frac{4}{5}\cdot\frac{1}{2}$ = 0·15	0·1590
22d.	9	21 ·51	7	9	4	5	5	$\frac{7}{8}\cdot\frac{9}{10}\cdot\frac{4}{5}\cdot\frac{5}{6}\cdot\frac{5}{6}$ = 0·44	0·2244
							Sum 1·20)	0·6815 (0·568 = mean.

Most probable mean of the whole, so obtained	= 9m 21s·568
Mean, similarly taken, but rejecting the results of the 18th and 21st as liable to suspicion	9 21 ·535
Arithmetical mean of all the four results	9 21 ·64
Arithmetical mean of the results of the four nights, obtained by the 2d combination, or from Capt. Sabine's and Mr. Herschel's observations alone	9 21 ·70
Arithmetical mean of the 3d combination, or Col. Bonne's and M. Largeteau's observations taken separately	9 21 ·69

On the whole then, 9m 21s·6 may be assumed as a result not very likely to be altered a whole tenth of a second, and very unlikely to be altered to twice that extent, by future determinations.

J. F. W. HERSCHEL.

London, November 2, 1825.

IX. *Observations on the poison of the common toad.* By JOHN DAVY, *M. D. F. R. S.*

Read December 22, 1825.

IN every country in which this animal is found, it is considered poisonous by the common people; and the opinion may be traced back to a very remote antiquity. Of late years the notion has been rejected by the professed naturalist, and placed in the number of vulgar prejudices. Thus, M. CUVIER speaking of the common toad, remarks: "Ce sont des animaux d'une forme hideuse, dégoûtante, que l'on accuse mal-à-propos d'être venimeux par leur salive, leur morsure leur urine, et même par l'humeur qu'il transpirent."*

In this, as in some other instances, the common and long received opinion is well founded, and that of the philosopher hastily and erroneously formed.

The poison of the common toad, I find, is seated chiefly in the integuments, in follicles in the cutis vera, beneath the cuticle and the coloured rete mucosum. These follicles are largest and most numerous near the shoulders, and about the neck of the animal; yet they are very generally distributed, and even on the extremities. Pressure being applied to the skin, a yellowish thick fluid exudes, and occasionally spurts to a considerable distance. It may be collected with ease in sufficient quantity for examination. It possesses, I

* Le regnè animal distribué d'après son organisation; tom. ii. p. 94. Paris, 1817.

have ascertained, the following properties. The greater part of it is soluble in alcohol and water. The aqueous solution is slightly viscid, and does not pass readily through a common philter. It is not precipitated by acetate of lead; and its transparency is very slightly impaired by corrosive sublimate. The substance obtained by evaporation, both from the aqueous and alcoholic solution, is light yellow, and transparent; has a faint and peculiar smell different from that of the toad, and it is slightly bitter, and very acrid, acting on the tongue like the extract of aconite prepared in vacuo, and even occasioning a smarting sensation when applied to the skin of the hand, and its effect lasts two or three hours. When heated, it readily melts, burns with a bright flame, and does not emit an ammoniacal odour. It is neither acid or alkaline, judging from its not changing the colour of litmus or turmeric paper. Caustic ammonia dissolves it; the solution remains acrid. Nitric acid also dissolves it; the solution is of a purple colour; neutralized by an alkali it is less acrid, as if partially decomposed. The small portion of the fluid, not soluble either in water or alcohol, and to which it owes its consistence, is probably a variety of albumen; and its appearance when burning would seem to warrant this idea.

Though this fluid of the toad is more acrid than the poison of the most venemous snakes, I do not find that it has any injurious, and much less fatal effect, when absorbed and carried into the circulation. A chicken punctured with a lancet dipped in it, received no injury: and in confirmation of this statement I may remark, that though, as already observed, it abounds chiefly in the integuments, it is not confined to

them; I have detected it in a notable quantity in the bile, in a minute quantity in the viscid fluid lubricating the tongue, and also in the urine, and even in the blood.

Reflecting on the use which this fluid may be of to the toad, it has occurred to me that it may answer two purposes, and these of importance to this abhorred but innocent reptile.

As the external surface of the skin is smeared with this "sweltered venom" (the very appropriate epithet given it by our great dramatic poet), it must serve to defend it against the attacks of carnivorous animals: "a toad to eat" is a proverbial expression well known; and the facts adduced show its propriety and force. I may here add, that nature has given this animal an additional security against attack, in providing it with integuments of great thickness, and strength, and hardness; which last-mentioned quality is imparted by a layer of cutis almost analogous to bone, immediately under the rete mucosum, abounding in phosphate and carbonate of lime, and carbonate of magnesia, semi-transparent, and yet so firm that it is not easily cut.

As the fluid contains a substance which is very inflammable, and as it may be considered excrementitious, though the blood is very slightly impregnated with it, it may serve to separate a portion of carbon from the blood; and thus in its formation be auxiliary to the function of the lungs. In support of this idea, I may remark, that I find the pulmonary arteries of the toad are each divided into two branches, one of which goes to its respective lung, and the other, very little smaller, to the cutis, between the head and shoulder, on each side, and is extensively ramified where the largest

venom-follicles are situated, and where there is a plexus of veins of great size, as if intended for a reservoir of blood.

This last-mentioned peculiarity of structure, and the situation of it, corresponding to the site of the gills of the tadpole, would seem to indicate, that the sub-cutaneous distribution of the second branch of the pulmonary artery, may aid the lungs farther in their office by bringing the blood to the surface to be acted on by atmospheric air.

I have endeavoured to ascertain if there is any direct communication by spiracula through the integuments. The results I have obtained have been negative. I have introduced air through a small incision, by means of a forcing syringe, under the loose skin, also into the cavity of the abdomen, and into the lungs by the superior glottis. The air has been very much compressed under water, yet it has been completely confined, not the smallest bubble having been forced through the skin, or the lungs.

When dried, the skin of the toad exhibits two kinds of pores; one kind, few in number, confined to the two tuberosities over the shoulders, sufficiently large to admit a hog's bristle; the other kind, very numerous, scattered over the whole surface, and very minute. They are both of them best seen by holding the skin between the eye and a strong light; the smallest appears as luminous points of a yellow hue; the largest as indistinct circles. Both are covered externally with transparent cuticle, and internally by a delicate surface of cellular tissue; some of the largest are also covered with rete mucosum; the smallest appears to be destitute of this membrane.

Whether these apparent pores are the medium of communicating between the blood in the sub-cutaneous capillary vessels, and the atmosphere; or, whether they are merely the cutaneous apparatus of secretion and exudation, it is difficult to determine; it is not improbable that they perform both functions.

Corfu, September 15th, 1825.

X. *On the magnetizing power of the more refrangible solar rays.* By *Mrs.* M. SOMERVILLE. *Communicated by* W. SOMERVILLE, *M. D. F. R. S. Feb.* 2. 1826.

Read February 2, 1826.

IN the year 1813, Professor MORICHINI of Rome discovered that steel, exposed to the violet rays of the solar spectrum, becomes magnetic. His experiments were repeated by Professor CONFIGLIACHI at Pavia, and also by Mons. BERARD, at Montpellier, without success. I am not aware of any one having attempted them in this country, perhaps from the belief that experiments which had sometimes failed in Italy, were not likely to succeed in our more northern climate. The unusual clearness of the weather last summer, however, induced me to try what could be accomplished in this country. Accordingly, in the month of July, an equiangular prism of flint glass, the three sides of which were each 1,4 by 1,1 inches, was fixed in a slit made to receive it in a window-shutter; by this prism a coloured spectrum was thrown on an opposite panel, at the distance of about five feet. I used for the subject of experiment, a very slender sewing needle an inch long, having previously ascertained that it was quite free from magnetism, by repeated exposure of both ends of it to the north and south pole of a very sensible magnetic needle, when it was found equally to attract either pole in every instance. The magnetic needle employed as a test in this experiment, is made of a sewing needle mag-

netised, and run through a small piece of cork, into which a conical cap of glass is inserted; the whole traverses on the point of a needle fixed perpendicularly in a stand.

I had no information at this time of the manner in which Professor MORICHINI had conducted his experiments; but it occurred to me that it was not likely that if the whole of the needle were equally exposed to the violet rays, the same influence should, at the same time, produce a south pole at one end of it, and a north pole at the other. I therefore covered half of the needle with paper, and fixed it to the panel with wax, between ten and eleven in the morning, in such a position that the uncovered part of it should be exposed to the violet rays. The needle was placed in a vertical plane, nearly perpendicular to the magnetic meridian, and inclined to the horizon. As I had not a heliostat, it was necessary to move the needle in a direction parallel to itself, to keep the exposed portion of it constantly in the violet ray.

The sun was bright at the time, and in less than two hours I had the gratification to find that the end of the needle which had been exposed to the violet rays attracted the south pole of the magnetic needle, and repelled the north pole. It had been previously ascertained that there was no iron near to disturb the results. The experiment was also repeated on the same day, under precisely similar circumstances, with the view of detecting any source of error that might have escaped observation in a first attempt; but the result was the same as in the first.

The season was so favourable that it afforded me daily opportunity of repeating the experiments, varying the size of the needles, always taking especial care to ascertain that

they were free from magnetism. The needles were placed in various directions in the plane of the magnetic meridian, sometimes in the angle of the dip, sometimes perpendicular to the magnetic meridian, and also at various angles with regard to it. In some cases the heads of the needles were exposed in place of the points, to the violet rays. Perhaps it might have been expected that the influence would have been greater in those instances in which the needles were placed in the plane of the magnetic meridian, and at the angle of the dip; and, consequently, polarity might have been expected to take place in a shorter time under these circumstances; yet in fact there seemed to be no difference; most of the needles became magnetic, some in longer, others in shorter periods, varying from about half an hour to four hours, but depending on circumstances which I have not yet been able to detect, further than that a number of results induced me to believe, that the experiments were more successful from ten to twelve, or one o'clock, than later in the day. The portion of the needle exposed was almost always a north pole, whether it pointed upwards or downwards. In a few instances in which the contrary occurred, it may possibly have arisen from some previous disposition in the needle to magnetism, too slight to be observed.

The distance of the needle from the prism was frequently varied by fixing the needle to the wooden pole of a fire-screen, but without material variation in the effect. I found it unnecessary to darken the room; it was sufficient to place the prism so as throw the spectrum on any place out of the sun's rays.

My next object was to endeavour to ascertain whether any

other of the more refrangible rays had the same property as the violet. A set of needles carefully examined as before, were therefore subjected to the different rays of the solar spectrum; the needles exposed to blue and green rays, sometimes acquired the magnetic property, though less frequently, and requiring longer exposure than when the violet rays were used; but the magnetism seemed to be equally strong in these as in the examples of the violet rays. The part exposed became a north pole. The indigo rays succeeded almost as well as the violet.

Pieces of clock and watch spring were next tried, under the idea that they might, possibly from their blue colour, be more susceptible of magnetic influence, and it was the case; their greater extent of surface however, or their softness, may have contributed to this susceptibility. The pieces of spring were from two to three inches long, and from the eighth to the half of an inch broad. It was difficult to procure watch and clock spring free from magnetism; it even happened on one occasion, that although the roll of spring was neutral, the pieces into which it was cut became magnetic; in one instance the pieces were heated so as to deprive them of magnetism and colour; they then required longer exposure to the rays in order to acquire polarity. Large bodkins were exposed to the violet rays without effect, the mass perhaps being too great. When needles already magnetic were tried, their magnetism was increased. Dr. WOLLASTON was so kind as to lend me a very large lens, having its centre covered with paper, which he had used in his investigations respecting the chemical rays. The lens concentrated the violet rays, and produced a magnetic effect in a shorter time

than the prism; but the rapid motion of the sun made it difficult to keep the needle in the focus. The effect was produced with equal facility by throwing the spectrum on the floor of the room; but success could not always be depended upon even when the weather seemed most favourable.

I now made the following experiments with blue glass. Three needles free from magnetism, having one half covered with paper, were laid horizontally on the stone outside of a south window, under a dark blue glass coloured by cobalt, in a very hot sun; after remaining in this position between three and four hours, they were found to have become feebly magnetic, the uncovered part being a north pole. On examining these needles the following day, they had lost their magnetism, a circumstance which had not before occurred, though it was observed sometimes to take place afterwards, as the force of the sun diminished from the advance of the season. There was no iron near, and the magnetic needle when placed on various parts of the stone, showed no magnetic influence in it. Next day the experiment was repeated with this difference, that the needles were left exposed to the sun, under the blue glass, six hours; and then the needles had not only acquired very sensible magnetism, but still retain it, at the distance of nearly six months. Pieces of clock spring, which had been heated as formerly mentioned, also became magnetic under the blue glass.

I was desirous of ascertaining whether this kind of glass suffered the chemical rays to pass, and thereby occasion these changes in the steel, therefore I employed a liquid holding muriate of silver in suspension, as a test, in the following manner: a piece of writing paper dipped in the liquid was

cut into two equal parts, of which one was placed under the blue glass, and the other under a white glass, as nearly at the same time as possible; but the one did not become black sooner than the other; nor on comparing them could any difference be perceived in intensity of colour, both having been equally exposed to the chemical rays. The experiment was repeated with the same result.

On the 26th of August, the thermometer at noon being 66°, two neutral pieces of clock spring were exposed to the sun, one under a thicker piece of the same blue glass, as in the former experiment, and the other under green glass; both acquired polarity.

31st of August, the thermometer at noon 68°. Having thus succeeded in producing magnetism under the circumstances described, I next tried the effect of exposing neutral pieces of clock spring to the sun, enveloped in violet and green silk. The half of each was covered with paper as before, and the pieces of spring then wrapped, one in green, and the other in violet-coloured ribbon, were fixed to the inside of a pane of glass in a window, where they were left exposed to the sun all day; in the evening both had become magnetic, although they were two of the pieces of spring already said to have acquired polarity more slowly from having been heated; and as before, the parts exposed to the sun under the ribbon were north poles.

To learn if heat had any share in producing magnetism in this case, I exposed three pieces of the same steel to a bright sunshine, on the 1st of September, the thermometer at noon being at 70°: one half of each was covered with paper, but

the other half had neither glass nor ribbon over it; and although the heat was greater than on the preceding day, no magnetism was produced.

On the 2d of September, thermometer at noon 68°, a piece of neutral white steel acquired polarity from exposure to the sun, enveloped in green ribbon, one half being covered with paper as before.

On the 3d of September, thermometer at noon 68°, two pieces of neutral spring became magnetic, one exposed in a violet-coloured ribbon, and the other in blue glass, while a similar piece of spring was in no way affected by exposure to white light: the half of each was covered with paper.

September the 4th, thermometer at noon 68°, five large sewing needles, two inches long, were exposed to the sun's rays, one in blue glass, one in green glass, one in violet ribbon, one in green ribbon, and one in white light; the half of each was covered with paper. Of all these two only became magnetic, namely, those in the blue glass, and in the violet ribbon.

On the 20th of September, thermometer at 69°, I placed pieces of steel enveloped in violet and green ribbon, and under glass of various colours, in different positions with regard to the magnetic meridian and dip. Several acquired polarity, the uncovered part being the north pole. A piece of steel became more strongly magnetic than usual, exposed in green ribbon, the position of which had been perpendicular to the horizon, and nearly in the magnetic meridian. For some time I still obtained similar results, though the magnetism became more feeble as the season advanced, from the

diminished force of the sun, in consequence of which further experiments were deferred till the return of summer shall afford me an opportunity of continuing them.

From the results which have been stated, I am induced to believe that the more refrangible rays of the solar spectrum have a magnetic influence even in this country.

XI. *On the mutual action of sulphuric acid and naphthaline, and on a new acid produced.* By M. FARADAY, *F. R. S. Corresponding Member of the Royal Academy of Sciences, &c. &c. Communicated January* 12, 1826.

Read February 16, 1826.

IN a Paper *on new compounds of carbon and hydrogen*, lately honoured by the Royal Society with a place in the Philosophical Transactions, I had occasion briefly to notice, the peculiar action exerted on certain of those compounds by sulphuric acid. During my attempts to ascertain more minutely the general nature of this action, I was led to suspect the occasional combination of the hydro-carbonaceous matter with the acid, and even its entrance into the constitution of the salts, which the acid afterwards formed with bases. Although this opinion proved incorrect, relative to the peculiar hydro-carbons forming the subject of that Paper, yet it led to experiments upon analogous bodies, and amongst others, upon naphthaline, which terminated in the production of the new acid body and salts now to be described.

Some of the results obtained by the use of the oil gas products are very peculiar. If, when completed, I find them sufficiently interesting, I shall think it my duty to place them before the Royal Society, as explicatory of that action of sulphuric acid which was briefly noticed in my last Paper.

Most authors who have had occasion to describe naphthaline, have noticed its habitudes with sulphuric acid.

Mr. BRANDE, several years since,* stated that naphthaline dissolved in heated sulphuric acid "in considerable abundance, forming a deep violet coloured solution, which bears diluting with water without decomposition. The alkalies produce in this solution a white flaky precipitate, and if diluted the mixture becomes curiously opalescent, in consequence of the separation of numerous small flakes." The precipitate by alkali was probably one of the salts to be hereafter described.

Dr. KIDD observes,† that "it blackens sulphuric acid when boiled with it; the addition of water to the mixture having no other effect than to dilute the colour, neither does any precipitation take place upon saturating the acid with ammonia."

Mr. CHAMBERLAIN states,‡ that sulphuric acid probably decomposes naphthaline, for that it holds but a very small quantity in solution. The true interpretation of these facts and statements will be readily deduced from the following experimental details.

1. *Production and properties of the new acid formed from sulphuric acid and naphthaline.*

Naphthaline, which had been almost entirely freed from naphtha by repeated sublimation and pressure, was pulverised; about one part with three or four parts by weight of cold sulphuric acid were put into a bottle, well shaken, and left for 36 hours. The mixture then contained a tenacious

* *Quarterly Journal of Science*, viii. 289, 1819.

† *Philosophical Transactions*, 1821, p. 216.

‡ *Annals of Philosophy*, N. S. vi. p. 136, 1823.

deep red fluid, and a crystalline solid; it had no odour of sulphurous acid. Water being added, all the liquid and part of the solid was dissolved; a few fragments of naphthaline were left, but the greater part was retained in solution. The diluted fluid being filtered was of a light brown tint, transparent, and of an acid and bitter taste.

For the purpose of combining as much naphthaline as possible with the sulphuric acid, 700 grains, with 520 grains of oil of vitriol were warmed in a Florence flask until entirely fluid, and were well shaken for about 30 minutes. The mixture was red; and the flask being covered up and left to cool, was found after some hours to contain, at the bottom, a little brownish fluid, strongly acid, the rest of the contents having solidified into a highly crystalline mass. The cake was removed, and its lower surface having been cleaned, it was put into another Florence flask with 300 grains more of naphthaline, the whole melted and well shaken together, by which a uniform mixture was obtained; but opaque and dingy in colour. It was now poured into glass tubes, in which it could be retained and examined without contact of air. In these the substance was observed to divide into two portions, which could easily be distinguished from each other, whilst both were retained in the fluid state. The heavier portion was in the largest quantity; it was of a deep red colour, opaque in tubes half an inch in diameter, but in small tubes could be seen through by a candle, or sun light, and appeared perfectly clear. The upper portion was also of a deep red colour, but clear, and far more transparent than the lower: the line of separation very defined. On cooling the tubes, the lighter substance first solidified, and after some

time the heavier substance also became solid. In this state, whilst in the tube, they could with great difficulty be distinguished from each other.

These two substances were separated, and being put into tubes, were further purified by being left in a state of repose at temperatures above their fusing points, so as to allow of separation; and when cold, the lower part of the lighter substance, and the upper, as well as the lower part of the heavier substance, were set aside for further purification.

The *heavier substance* was a red crystalline solid, soft to the nail like a mixture of wax and oil. Its specific gravity was from 1.3 to 1.4, varying in different specimens; its taste sour, bitter, and somewhat metallic. When heated in a tube, it fused, forming as before a clear but deep red fluid. Further heat decomposed it, naphthaline, sulphurous acid, charcoal, &c. being produced. When heated in the air it burnt with much flame. Exposed to air it attracted moisture rapidly, became brown and damp upon the surface, and developed a coat of naphthaline. It dissolved entirely in alcohol, forming a brown solution. When rubbed in water a portion of naphthaline separated, amounting to 27 per cent. and a brown acid solution was obtained. This was found by experiments to contain a peculiar acid mixed with a little free sulphuric acid, and it may conveniently be called *the impure acid.*

The *lighter substance* was much harder than the former, and more distinctly crystalline. It was of a dull red colour, easily broken down in a mortar, the powder being nearly white, and adhesive like naphthaline. It was highly sapid, being acid, bitter, and astringent. When heated in a tube it melted, forming a clear red fluid, from which by a continued

heat much colourless naphthaline sublimed, and a black acid substance was left, which at a high temperature gave sulphurous acid and charcoal. When heated in the air it took fire and burnt like naphthaline. Being rubbed in a mortar with water, a very large portion of it proved to be insoluble; this was naphthaline; and on filtration the solution contained the peculiar acid found to exist in the *heavier substance*, contaminated with very little sulphuric acid. More minute examination proved that this *lighter substance* in its fluid state was a solution of a small quantity of the dry peculiar acid in naphthaline; and that the *heavier substance* was an union of the peculiar acid in large quantity with water, free sulphuric acid, and naphthaline.

It was easy by diminishing the proportion of naphthaline to make the whole of it soluble, so that when water was added to the first result of the experiment, nothing separated; and the solution was found to contain sulphuric acid with the peculiar acid. But reversing the proportions, no excess of naphthaline was competent, at least in several hours, to cause the entire disappearance of the sulphuric acid. When the experiment was carefully made with pure naphthaline, and either at common, or slightly elevated temperatures, no sulphurous acid appeared to be formed, and the action seemed to consist in a simple union of the concentrated acid and the hydro-carbon.

Hence it appears, that when concentrated sulphuric acid and naphthaline are brought into contact at common, or moderately elevated temperatures, a peculiar compound of sulphuric acid with the elements of the naphthaline is produced, which possesses acid properties; and as this exists in large

quantity in the heavier of the bodies above described, that product may conveniently be called the *impure solid acid*. The experiments made with it, and the mode of obtaining the pure acid from it, are now to be described.

Upon applying heat and agitation to a mixture of one volume of water, and five volumes of impure solid acid, the water was taken up to the exclusion of nearly the whole of the free naphthaline present; the latter separating in a colourless state from the red hydrated acid beneath it. As the temperature of the acid diminished, crystallization in tufts commenced here and there, and ultimately the whole became a brownish yellow solid. A sufficient addition of water dissolved nearly the whole of this hydrated acid, a few flakes only of naphthaline separating.

A portion of the impure acid in solution was evaporated at a moderate temperature; when concentrated, it gradually assumed a light brown tint. In this state it became solid on cooling, of the hardness of cheese, and was very deliquescent. By further heat it melted, then fumed, charred, &c. and gave evidence of the abundant presence of carbonaceous matter.

Some of the impure acid in solution was neutralized by potash, during which no naphthaline or other substance separated. The solution being concentrated until ready to yield a film on its surface, was set aside whilst hot to crystallize: after some hours the solution was filled with minute silky crystals, in tufts, which gave the whole, when stirred, not the appearance of mixed solid salt and liquid, but that of a very strong solution of soap. The agitation also caused the sudden solidification of so much more salt, that the whole

became solid, and felt like a piece of soft soap. The salt when dried had no resemblance to sulphate of potash. When heated in the air, it burnt with a dense flame, leaving common sulphate of potash, mixed with some sulphuret of potassum, resulting from the action of the carbon, &c. upon the salt.

Some of the dry salt was digested in alcohol to separate common sulphate of potash. The solution being filtered and evaporated, gave a white salt soluble in water and alcohol, crystalline, neutral, burning in the air with much flame, and leaving sulphate of potash. It was not precipitated by nitrate of lead, muriate of baryta, or nitrate of silver.

It was now evident that an acid had been formed peculiar in its nature and composition, and producing with bases peculiar salts. In consequence of the solubility of its barytic salt, the following process for the preparation of the pure acid was adopted.

A specimen of native carbonate of baryta was selected, and its purity ascertained. It was then pulverized, and rubbed in successive portions with a quantity of the impure acid in solution, until the latter was perfectly neutralized, during which the slight colour of the acid was entirely removed. The solution was found to contain the peculiar barytic salt. Water added to the solid matter dissolved out more of the salt; and ultimately only carbonate and sulphate of baryta, mixed with a little of another barytic salt, remained. The latter salt being much less soluble in water than the former, was not removed so readily by lixiviation, and was generally found to be almost entirely taken up by the last portions of water applied with heat.

The barytic salt in solution was now very carefully decomposed, by successive additions of sulphuric acid, until all the baryta was separated, no excess of sulphuric acid being permitted. Being filtered, a pure aqueous solution of the peculiar acid was obtained. It powerfully reddened litmus paper, and had a bitter acid taste. Being evaporated to a certain degree, a portion of it was subjected to the continued action of heat; when very concentrated it began to assume a brown colour, and on cooling became thick, and ultimately solid, and was very deliquescent. By renewed heat it melted, then began to fume, charred, but did not flame; and ultimately gave sulphuric and sulphurous acid vapours, and left charcoal.

Another portion of the unchanged strong acid solution was placed over sulphuric acid in an exhausted receiver. In some hours it had by concentration become a soft white solid, apparently dry; and after a longer period was hard and brittle. In this state it was deliquescent in the air, but in close vessels underwent no change in several months. Its taste was bitter, acid, and accompanied by an after metallic flavour, like that of cupreous salts. When heated in a tube at temperatures below 212°, it melted without any other change, and on being allowed to cool, crystallized from centres, the whole ultimately becoming solid. When more highly heated, water at first passed off, and the acid assumed a slight red tint; but no sulphurous acid was as yet produced, nor any charring occasioned; and a portion being dissolved and tested by muriate of baryta, gave but a very minute trace of free sulphuric acid. In this state it was probably anhydrous. Further heat caused a little naphthaline

to rise, the red colour became deep brown, and then a sudden action commenced at the bottom of the tube, which spread over the whole, and the acid became black and opaque. Continuing the heat, naphthaline, sulphurous acid, and charcoal were evolved; but even after some time the residuum examined by water and carbonate of baryta, was found to contain a portion of the peculiar acid undecomposed, unless the temperature had been raised to redness.

These facts establish the peculiarity of this acid, and distinguish it from all others. In its solid state it is generally a hydrate containing much combustible matter. It is readily soluble in water and alcohol, and its solutions forms neutral salts with bases, all of which are soluble in water, most of them in alcohol, and all combustible, leaving sulphates or sulphurets according to circumstances. It dissolves in naphthaline, oil of turpentine, and olive oil, in greater or smaller quantities, according as it contains less or more water. As a hydrate, when it is almost insoluble in naphthaline, it resembles the *heavier substance* obtained as before described, by the action of sulphuric acid on naphthaline, and which is the solid hydrated acid, containing a little naphthaline, and some free sulphuric acid; whilst the *lighter substance* is a solution of the dry acid in naphthaline; the water present in the oil of vitriol originally used being sufficient to cause a separation of a part, but not of the whole.

2. *Salts formed by the peculiar acid with bases.*

These compounds may be formed, either by acting on the bases or their carbonates by the pure acid, obtained as already described; or the impure acid in solution may be used,

the salts resulting being afterwards freed from sulphates, by solution in alcohol. It is however proper to mention that another acid, composed of the same elements, is at the same time formed with the acid in question, in small, but variable proportions. The impure acid used, therefore, should be examined as to the presence of this body, in the way to be directed when speaking of the barytic salts; and such specimens as contain very little or none of it should be selected.

Potash forms with the acid a neutral salt, soluble in water and alcohol, forming colourless solutions. These yield either transparent or white pearly crystals, which are soft, slightly fragile, feel slippery between the fingers, do not alter by exposure to air, and are bitter and saline to the taste. They are not very soluble in water; but they undergo no change by repeated solutions and crystallizations, or by long continued ebullition. The solutions frequently yield the salt in acicular tufts, and they often vegetate, as it were, by spontaneous evaporation, the salt creeping over the sides of the vessel, and running to a great distance in very beautiful forms. The solid salt heated in a tube gave off a little water, then some naphthaline; after that a little carbonic and sulphurous acid gases arose, and a black ash remained, containing carbon, sulphate of potash, and sulphuret of potassium. When the salt was heated on platinum foil, in the air, it burnt with a dense flame, leaving a slightly alkaline sulphate of potash.

Soda yields a salt, in most properties resembling that of potash; crystalline, white, pearly, and unaltered in the air.

I thought that, in it, the metallic taste which frequently occurred with this acid and its compounds was very decided. The action of heat was the same as before.

Ammonia formed a neutral salt imperfectly crystalline, not deliquescent, but drying in the atmosphere. Its taste was saline and cooling. It was readily soluble in water and alcohol. When heated on platinum foil it fused, blackened, burnt with flame, and left a carbonaceous acid sulphate of ammonia, which by further heat was entirely dissipated. Its general habits were those of ammoniacal salts. When its solutions, though previously rendered alkaline, were evaporated to dryness at common temperatures, and exposed to air, the salt became strongly acid to litmus paper. This however is a property common to all soluble ammoniacal salts, I believe, without exception.

Baryta. It is easy by rubbing carbonate of baryta with solution of the impure acid, to obtain a perfectly neutral solution, in which the salt of baryta, containing the acid already described, is very nearly pure. There is in all cases an undissolved portion, which being washed repeatedly in small quantities of hot water, yields to the first portions a salt, the same as that in the solution. As the washings proceed, it is found, that the salt obtained does not burn with so much flame on platina foil, as that at first separated; and the fifth or sixth washing will perhaps separate only a little of a salt, which when heated in the air, in small quantities, burns without flame in the manner of tinder. Hence it is evident that there are two compounds of baryta, which as they are both soluble in water, both neutral, and both combustible,

leaving sulphate of baryta, differ probably only in the quantity of combustible matter present, or its mode of combination in the acid.

It is this circumstance, of the formation of a second salt in small but variable quantities with the first, which must be guarded against, as before mentioned, in the preparation of salts from the impure acid. It varies in quantity according to the proportions of materials, and the heat employed: and I have thought that, when the naphthaline has been in large quantity, and the temperature low, the smallest quantity is produced. When the impure acid is used for the preparation of the salts now under description, a small portion of it should be examined by carbonate of baryta, as above, and rejected, if it furnish an important quantity of the flameless salt.

These bodies may be distinguished from each other provisionally, as the *flaming* and the *glowing* salts of baryta, from their appearances when heated in the air. The latter is more distinctly crystalline than the former, and much less soluble, which enabled me by careful and repeated crystallizations, to obtain both in their pure states.

The *flaming* salt (that corresponding to the acid now under description) when obtained by the slow evaporation of the saturated solution, formed tufts, which were imperfectly crystalline. When drops were allowed to evaporate on a glass plate, the crystalline character was also perceived; but when the salt was deposited rapidly from its hot saturated solution, it appeared in the form of a soft granular mass. When dry, it was white and soft, not changing in the atmosphere. It was readily soluble in water and alcohol, but was

not affected by ether. Its taste was decidedly bitter. When heated in the air on platinum foil it burnt with a bright smoky flame, like naphthaline, sending flocculi of carbon into the atmosphere, and leaving a mixture of charcoal, sulphuret of barium, and sulphate of baryta.

After being heated to 212° for some time, the salt appeared to be perfectly dry, and in that state was but very slightly hygrometric. When heated in a tube naphthaline was evolved; but the substance could be retained for hours at a temperature of 500° F. before a sensible portion of naphthaline had separated: a proof of the strength of the affinity by which the hydro-carbon was held in combination. When a higher temperature was applied, the naphthaline, after being driven off, was followed by a little sulphurous acid, a small portion of tarry matter, and a carbonaceous sulphate and sulphuret were left.

This salt was not affected by moderately strong nitric or nitro-muriatic acid, even when boiled with them; and no precipitation of sulphate took place. When the acids were very strong, peculiar and complicated results were obtained. When put into an atmosphere of chlorine, at common temperatures, it was not at all affected by it. Heat being applied, an action between the naphthaline evolved, and chlorine, such as might be expected, took place.

When a strong solution of the pure acid was poured into a strong solution of muriate of baryta, a precipitate was formed, in consequence of the production of this salt. It was re-dissolved by the addition of water. The fact indicates that the affinity of this acid for baryta is stronger than that of muriatic acid.

The *second*, or *glowing* salt of baryta, was obtained in small crystalline groups. The crystals were prismatic, colourless, and transparent: they were almost tasteless, and by no means so soluble either in hot or cold water as the former salts. They were soluble in alcohol, and the solutions were perfectly neutral. When heated on platinum foil they gave but very little flame, burning more like tinder, and leaving a carbonaceous mixture of sulphuret and sulphate. When heated in a tube they gave off a small quantity of naphthaline, some empyreumatic fumes, with a little sulphurous acid, and left the usual product.

This salt seemed formed in largest quantity when one volume of naphthaline and two volumes of sulphuric acid were shaken together, at a temperature as high as it could be without charring the substances. The tint, at first red, became olive green; some sulphurous acid was evolved, and the whole would ultimately have become black and charred, had it not been cooled before it had proceeded thus far, and immediately dissolved in water. A solution was obtained, which though dark itself, yielded, when rubbed with carbonate of baryta, colourless liquids; and these when evaporated furnished a barytic salt, burning without much flame, but which was not so crystalline as former specimens. No attempt to form the glowing salt from the flaming salt by solution of caustic baryta, succeeded.

Strontia. The compound of this earth with the acid already described very much resembled the flaming salt of baryta. When dry it was white, but not distinctly crystalline: it was soluble in water and alcohol; not alterable in the air, but

when heated burnt with a bright flame, without any red tinge, and left a result of the usual kind.

Lime gave a white salt of a bitter taste, slightly soluble in water, soluble in alcohol, the solutions yielding imperfect crystalline forms on evaporation: it burnt with flame; and both in the air and in tubes, when heated, gave results similar to those of the former salts.

Magnesia formed a white salt with a moderately bitter taste; crystallizing in favourable circumstances, burning with flame, and giving such results by the action of heat as might be expected.

Iron. The metal was acted upon by the acid, hydrogen being evolved. The moist protoxide being dissolved in the acid gave a neutral salt capable of crystallization. This by exposure to air slowly acquired oxygen, and a portion of per-salt was found.

Zinc was readily acted upon by the acid, hydrogen evolved, and a salt formed. The same salt resulted from the action of the acid upon the moist oxide. It was moderately soluble in hot water, the solution on cooling affording an abundant crop of acicular crystals. The salt was white and unchangeable in the air; its taste bitter. It burnt with flame, and gave the usual results by heat.

Lead. The salt of this metal was white, solid, crystalline, and soluble in water and alcohol. It had a bitter metallic taste, with very little sweetness. The results by heat were such as might be expected.

Manganese. The protoxide of this metal formed a neutral crystalline salt with the acid. It had a slightly austere taste,

was soluble in water and alcohol, and was decomposed by heat, with the general appearances already described.

Copper. Hydrated per-oxide of copper formed an acid salt with the acid, and the solution evaporated in the air left radiated crystalline films. The dry salt when heated fused, burnt with flame, and exhibited the usual appearances.

Nickel. The salt of this metal was made from the moist carbonate. It was soluble, crystalline, of a green colour, and decomposed by heat in the usual manner. In one instance an insoluble sub-salt was formed.

Silver. Moist carbonate of silver dissolved readily in the acid, and a solution, almost neutral, was quickly obtained. It was of a brown colour, and a powerful metallic taste. By evaporation it gave a splendent, white, crystalline salt; not changing in the air except when heated; but then, burning with flame and ultimately leaving pure silver. When the solution of the salt was boiled for some time, a black insoluble matter was thrown down, and a solution obtained, which by evaporation gave abundance of a yellow crystalline salt. The changes which took place during the action of heat in the moist way were not minutely examined.

Mercury. Moist proto-carbonate of mercury dissolved in the acid forming a salt not quite neutral, crystallizing feebly in the air, white, of a metallic taste, not deliquescent, and decomposed with various phenomena by heat. By re-solution in water or alcohol, and heat, a sub-salt of a yellow colour was formed.

The moist hydrated per-oxide of mercury also dissolved in the acid, forming an acid solution, which by evaporation

gave a yellowish deliquescent salt, decomposed by heat, burning in the air, and entirely volatile.

3. *Analysis of the acid and salts.*

When solution of the pure acid was subjected to the voltaic battery, oxygen and hydrogen gases were evolved in their pure state: no solid matter separated, but the solution became of a deep yellow colour at the positive pole, occasioned by the evolution of free sulphuric acid, which re-acted upon the hydro-carbon. A solution of the barytic salts gave similar results.

The analytical experiments upon the composition of this acid and its salts were made principally with the compound of baryta. This was found to be very constant in composition, could be obtained anhydrous at moderate temperatures, and yet sustained a high temperature before it suffered any change.

A portion of the pure salt was prepared and dried for some hours on the sand bath, at a temperature about 212°. Known weights were then heated in a platinum crucible to dissipate and burn off the combustible matter; and the residuum being moistened with sulphuric acid to decompose any sulphuret of barium formed, was heated to convert it into a pure sulphate of baryta. The results obtained were very constant, and amounted to 41.714 of sulphate of baryta per cent. of salt used, equivalent to 27.57 baryta per cent.

Other portions of the salt were decomposed by being heated in a flask with strong nitro-muriatic acid, so as to liberate the sulphuric acid from the carbon and hydrogen present, and yet retain it in the state of acid. Muriate of

baryta was then added, the whole evaporated to dryness, heated red-hot, washed with dilute muriatic acid to remove the baryta uncombined with sulphuric acid, and the sulphate collected, dried, and weighed. The results were inconstant; but the sulphate of baryta obtained, always much surpassed that furnished by the former method. Judging from this circumstance that the sulphuric acid in the salt was more than an equivalent for the baryta present, many processes were devised for the determination of its quantity, but were rejected in consequence of difficulties and imperfections, arising, principally, from the presence and action of so much carbonaceous matter. The following was ultimately adopted.

A quantity of per-oxide of copper was prepared by heating copper plates in air and scaling them. A sufficient quantity of pure muriatic and nitric acids were provided, and also a specimen of pure native carbonate of baryta. Seven grains of the salt to be examined were then mixed with seven grains of the pulverized carbonate of baryta, and afterwards with 312 grains of the oxide of copper. The mixture being put into a glass tube was successively heated throughout its mass, the gas liberated being passed through a mixture of baryta water and solution of muriate of baryta. It was found that no sulphurous or sulphuric acids came off, or indeed sulphur in any state. The contents of the tube were then dissolved in an excess of nitric and muriatic acids, above that required to take up all that was soluble; and a little solution of muriate of baryta was added for the sake of greater certainty. A portion of sulphate of baryta remained undissolved, equivalent to the sulphuric acid of the salt experimented upon, with that contained accidentally in the oxide of

copper acids, &c. This sulphate was collected, washed, dried and weighed. Similar quantities of the carbonate of baryta and oxide of copper were then dissolved in as much of the nitric and muriatic acids as was used in the former experiment; and the washings and other operations being repeated exactly in the same way, the quantity of sulphate of baryta occasioned by the presence of sulphuric acid in the oxide, acids, &c. was determined. This, deducted from the weight afforded in the first experiments, gave the quantity produced from the sulphuric acid actually existing in the salt. Experiments so conducted gave very uniform results. The mean of many, indicated 8.9 grains of sulphate of baryta for 10 grains of salt used, or 89 grains per cent. equivalent to 30.17 of sulphuric acid for every 100 of salt decomposed.

In the analytical experiments, relative to the quantity of carbon and hydrogen contained in the salt, a given weight of the substance being mixed with per-oxide of copper, was heated in a green glass tube. The apparatus used consisted of Mr. COOPER's lamp furnace, with Dr. PROUT's mercurial trough; and all the precautions that could be taken, and which are now well known, were adopted for the purpose of obtaining accurate results. When operated upon in this way, the only substances evolved from the salt, were carbonic acid and water. As an instance of the results, 3.5 grains of the salt afforded 11.74 cubic inches of carbonic acid gas, and 0.9 of a grain of water. The mean of several experiments gave 32.93 cubic inches of carbonic acid gas, and 2.589 grains of water, for every 10 grains of salt decomposed.

On these data, 100 grains of the salt would yield 329.3

cubic inches of carbonic acid, or 153.46 grains, equivalent to 41.9 grains of carbon, and 25.89 grains of water, equivalent to 2.877 grains of hydrogen. Hence 100 grains of the salt yielded

Baryta	-	27.57	78
Sulphuric acid	-	30.17	85.35
Carbon	-	41.90	118.54
Hydrogen	-	2.877	8.13
		102.517	

In the second numerical column the experimental results are repeated, but increased, that baryta might be taken in the quantity representing one proportional, hydrogen being unity: and it will be seen that they do not differ far from the following theoretical statement.

Baryta	1	proportional	78
Sulphuric acid	2	ditto	80
Carbon	20	ditto	120
Hydrogen	8	ditto	8

The quantity of sulphuric acid differs most importantly from the theoretical statement, and it probably is *that* element of the salt, in the determination of which most errors are involved. The quantity of oxide of copper and of acids required to be used in that part of the analysis, may have introduced errors, affecting the small quantity of salt employed, which when multiplied, as in the deduction of the numbers above relative to 100 parts, may have created an error of that amount.

As there is no reason to suppose that during the combination of the acid with the baryta any change in its proportions

takes place, the results above, *minus* the baryta, will represent its composition: from which it would appear, that one proportional of the acid consists of two proportionals of sulphuric acid, twenty of carbon, and eight of hydrogen; these constituents forming an acid equivalent in saturating power to one proportional of other acids. Hence it would seem, that half the sulphuric acid present, at least when in combination, is neutralized by the hydro-carbon; or, to speak in more general terms, that the hydro-carbon has diminished the saturating power of the sulphuric acid to one half. This very curious and interesting fact in chemical affinity was however made known to me by Mr. HENNELL of Apothecaries' Hall, as occurring in some other compounds of sulphuric acid and hydro-carbon, before I had completed the analysis of the present acid and salts; and a similar circumstance is known with regard to muriatic acid, in the curious compound discovered by M. KIND, which it forms with oil of turpentine. Mr. HENNELL is I believe on the point of offering an account of his experiments to the Royal Society, and as regards date they precede mine.

It may be observed, that the existence of sulphuric acid in the new compounds, is assumed, rather than proved; and that the non-appearance of sulphurous acid, when sulphuric acid and naphthaline act on each other, is not conclusive as to the non-reaction of the bodies. It is possible that part of the hydrogen of the naphthaline may take oxygen from one of the proportions of the sulphuric acid, leaving the hyposulphuric acid of WELTER and GAY LUSSAC, which with the hydro-carbon may constitute the new acid. I have not time at present to pursue these refinements of the subject, or to

repeat the analyses which have been made of naphthaline, and which would throw light upon the question. Such a view would account for a part of the overplus in weight, but not for the excess of the sulphuric acid obtained, above two proportionals.

The glowing salt of baryta was now analysed by a process similar to that adopted for the flaming salt. The specimen operated upon was pure, and in a distinctly crystalline state. It had been heated to about 440° F. for three hours in a metallic bath. Ten grains of this salt exposed to air for 40 hours increased only 0.08 of a grain in weight. These when converted into sulphate of baryta by heat and sulphuric acid, gave 4.24 grains. Seven grains by carbonate of baryta, oxide of copper, heat, &c. gave 6.02 grains of sulphate of baryta: hence 10 grains of the salt would have afforded 8.6 grains of the sulphate equivalent to 2.915 grains of sulphuric acid. Five grains when heated with oxide of copper gave 16.68 cubic inches of carbonic acid gas, equal to 7.772 grains, and equivalent to 2.12 grains of carbon. The water formed amounted to 1.2 grains equivalent to 0.133 of a grain of hydrogen.

From these data, 100 grains of the salt would appear to furnish

Baryta -	28.03	-	78 or 1 proportional.
Sulphuric acid	29.13	-	81.41 nearly two proportionals.
Carbon -	42.40	-	118. approaching to 20 ditto.
Hydrogen	2.66	-	7.4 or 7.4 proportionals.
	102.22		

results not far different from those obtained with the former salt.

I have not yet obtained sufficient quantities of this salt in a decidedly crystalline state to enable me satisfactorily to account for the difference between it and the flaming salt.

Attempts were made to form similar compounds with other acids than the sulphuric. Glacial phosphoric acid was heated and shaken in naphthaline, but without any particular results. A little water was then used with another portion of the materials, to bring the phosphoric acid into solution, but no decided combination could be obtained. Muriatic acid gas was brought into contact with naphthaline in various states, and at various temperatures, but no union could be effected either of the substances or their elements.

Very strong solution of potash was also heated with naphthaline, and then neutralized by sulphuric acid; nothing more however than common sulphate of potash resulted.

As the appropriation of a name to this acid will much facilitate future reference and description, I may perhaps be allowed to suggest that of *sulpho-naphthalic acid*, which sufficiently indicates its source and nature without the inconvenience of involving theoretical views.

Royal Institution, January 10, 1826.

XII. *On the nervous circle which connects the voluntary muscles with the brain.* *By* CHARLES BELL, *Esq.* *Communicated by the President, January* 25, 1826.

Read February 16, 1826.

IN the Papers which I have had the honour of addressing to the Society on the arrangement of the nerves of the human body, I have proceeded upon a comparison of the nerves of the spinal marrow with the nerves of the encephalon.

It was shown that the former were compounded of filaments possessing different powers, and that each nerve, having several properties or endowments collected within itself, proceeded to its destination without intricacy.

Unless we had discovered the composition of the roots of these nerves, we should have continued to suppose that one nerve was simple in its structure, and yet capable of bestowing the very different properties of motion and sensation.

But having satisfied myself that the roots of the spinal nerves had distinct powers, I followed up the columns of the spinal marrow; and with a knowledge of the composition of these nerves as a key, I examined the different properties of the nerves of the encephalon. Here, in the head, the nerves arise simply, and diverge to their destinations without the close compact or union which the spinal nerves form; and accordingly, the anatomy of these nerves of the brain affords satisfactory proof of their uses or functions. I am about to

show that every muscle has two nerves, of different properties, supplied to it. This I could not have ascertained by examination of the spinal nerves alone, because of the intimate union of all their fibres; I had recourse therefore to the nerves of the head. By prosecuting those enquiries, which led to the distinction of the different classes of nerves, I hope now to demonstrate—*that where nerves of different functions take their origin apart and run a different course: two nerves must unite in the muscles, in order to perfect the relations betwixt the brain and these muscles.*

It may be in the recollection of the Society, that my first Paper showed the difference of the nerves of the face; by dividing one nerve, sensation was destroyed, whilst motion remained; and by dividing the other, motion was stopped, whilst sensibility remained entire.

Other parts of the nervous system since that time have engaged my attention; and it is only now that I am able to make full use of the facts announced in my first Paper, which were indeed expected to lead to further improvement of our knowledge of the animal œconomy. When I distinguished the two classes of nerves going to the muscles of the face, and divided the motor nerve, and when the muscles were deprived of motion by this experiment, the natural question suggested itself—of what use are the nerves that remain entire?

For a time I believed that the fifth nerve, which is the sensitive nerve of the head and face, did not terminate in the substance of the muscles, but only passed through them to the skin; and I was the more inclined to this belief on observing, that the muscular parts when exposed in surgical

operations did not possess that exquisite sensibility which the profusion of the sensitive nerves would imply, or which the skin really possesses.

Still dissection did not authorise this conclusion. I traced the sensitive nerves into the substance of the muscles: I found that the fifth pair was distributed more profusely to the muscles than to the skin; and that estimating all the nerves given to the muscles, the greater proportion belonged to the fifth or sensitive nerve, and the smaller proportion to the seventh or motor nerve. On referring to the best authorities, as MECKEL,* and my excellent preceptor MONRO, the extremities of the fifth were described by them as going into the muscles, so that of this fact there cannot be a doubt.

Having in a former Paper demonstrated that the portio dura of the seventh nerve was the motor of the face, and that it run distinct from the sensitive nerve, the fifth, and observing that they joined at their extremities, or plunged together into the muscles, I was nevertheless unwilling to draw a conclusion from a single instance; and therefore cast about for other examples of the distribution of the muscular nerves. It was easy to find motor nerves in combination with sensitive nerves, for all the spinal nerves are thus composed; but we wanted a muscular nerve clear in its course, to see what alliance it would form in its ultimate distribution in the muscle. I found in the lower maxillary nerve the example I required.

The fifth pair, from which this lower maxillary nerve comes, as I have elsewhere explained, is a compound nerve; that is to say, it is composed of a nerve of sensation, and a

* MECKEL de quinto pare nervorum cerebri.

nerve of motion. It arises in two roots, one of these is the muscular nerve, the other the sensible nerve; on this last division the Gasserian ganglion is formed. But we can trace the motor nerve clear of the ganglion and onward in its course to the muscles of the jaws, and so it enters the temporal masseter pterygoid and buccinator muscles.

If all that is necessary to the action of a muscle be a nerve to excite to contraction, these branches should have been unaccompanied; but on the contrary, I found that before these motor nerves entered the several muscles, they were joined by branches of the nerves which came through the Gasserian ganglion, and which were sensitive nerves.

I found the same result on tracing motor nerves into the orbit, and that the sensitive division of the fifth pair of nerves was transmitted to the muscles of the eye, although these muscles were supplied by the third, fourth, and sixth nerves.

A circumstance observed on minute dissection remained unexplained,—when motor nerves are proceeding to several muscles they form a plexus; that is, an interlacement and exchange of fibres takes place.

The muscles have no connection with each other, they are combined by the nerves; but these nerves, instead of passing betwixt the muscles, interchange their fibres before their distribution to them, and by this means combine the muscles into classes. The question therefore may thus be stated: why are nerves, whose office it is to convey sensation, profusely given to muscles in addition to those motor nerves which are given to excite their motions? and why do both classes of muscular nerves form plexus?

To solve this question, we must determine whether muscles

have any other purpose to serve than merely to contract under the impulse of the motor nerves. For if they have a reflective influence, and if their condition is to be felt or perceived, it will presently appear that the motor nerves are not suitable internuncii betwixt them and the sensorium.

I shall first enquire, if it be necessary to the governance of the muscular frame, that there be a consciousness of the state or degree of action of the muscles? That we have a sense of the condition of the muscles, appears from this: that we feel the effects of over exertion and weariness, and are excruciated by spasms, and feel the irksomeness of continued position. We possess a power of weighing in the hand:—what is this but estimating the muscular force? We are sensible of the most minute changes of muscular exertion, by which we know the position of the body and limbs, when there is no other means of knowledge open to us. If a rope-dancer measures his steps by the eye, yet on the other hand a blind man can balance his body. In standing, walking, and running, every effort of the voluntary power, which gives motion to the body, is directed by a sense of the condition of the muscles, and without this sense we could not regulate their actions.

If it were necessary to enlarge on this subject, it would be easy to prove that the muscular exertions of the hand, the eye, the ear, and the tongue, are felt and estimated when we have perception through these organs of sense; and that without a sense of the actions of the muscular frame, a very principal inlet to knowledge would be cut off.

If it be granted, that there must be a sense of the condition of the muscle, we have next to show that a motor nerve is

not a conductor towards the brain, and that it cannot perform the office of a sensitive nerve.

Without attempting to determine the cause, whether depending on the structure of the nervous cord, or the nature, or the source of the fluid contained, a pure or simple nerve has the influence propagated along it in one direction only, and not backwards and forwards; it has no reflected operation or power retrograde; it does not both act from and to the sensorium.

Indeed reason without experience would lead us to conclude, that whatever may be the state, or the nature of the activity of a motor nerve during exertion, it supposes an energy proceeding *from* the brain *towards* the muscles, and precludes the activity of the same nerve in the opposite direction at the same moment. It does not seem possible therefore that a motor nerve can be the means of communicating the condition of the muscles to the brain.

Expose the two nerves of a muscle; irritate one of them, and the muscle will act; irritate the other, and the muscle remains at rest. Cut across the nerve which had the power of exciting the muscle, and stimulate the one which is undivided—the animal will give indication of pain; but although the nerve be injured so as to cause universal agitation, the muscle with which it is directly connected does not move. Both nerves being cut across, we shall still find that by exciting one nerve the muscle is made to act, even days after the nerve has been divided; but the other nerve has no influence at all.

Anatomy forbids us to hope that the experiment will be

as decisive when we apply the irritants to the extremities of the divided nerves which are connected with the brain; for all the muscular nerves receive more or less minute filaments of sensitive nerves, and these we can trace into them by the knife, and consequently, they will indicate a certain degree of sensibility when hurt. To expose these nerves near their origins, and before any filament of a sensitive nerve mingles with them, requires the operator to cut deep, to break up the bones, and to divide the blood-vessels. All such experiments are much better omitted; they never can lead to satisfactory conclusions.

Experience on the human subject most abundantly illustrates these facts. For example:—a patient of mine having, by a tumour pressing the nerves of the orbit, lost the sensibility of the eye and eye-lids, she retained the motion of the eye-lids by the portio dura coming round externally and escaping the pressure which injured the other nerves. Here the course of sensibility backwards to the brain was cut off, while the course of volition was free; she could not tell whether the eye-lid was open or shut, but being asked to shut the eye which was already closed, she acted with the orbicular muscle and puckered the eye-lids. When I touched the eye there was no winking, because the sensitive fifth pair had lost its power, although she could command the motion by voluntary exertion.

In another instance, when the eye was insensible, touching the eye gave rise to a blush of redness and to inflammation, because the *part* was excited, but the muscles were not called into action: the relations which connect the sensibility of the eye with the motions of the eye and eyelid are established

in the roots of the fifth and seventh in the brain; the loss of function of the fifth nerve therefore interrupted the circle. Here too the motor nerve of the eye-lid was perfect, and the eye-lid readily acted under the influence of the will, but when the eye-lid was touched or pricked it communicated no sensation. Is this insensibility of a motor nerve owing to the course of its influence being from the brain, and not towards it? When the nostril had lost its sensibility from an affection of the fifth pair, we could not excite sneezing; when the tongue and cheek had lost sensibility, the morsel was permitted to remain between the tongue and the cheek until it was offensive, although the motions both of the tongue and the cheek were perfect. All these phenomena correspond with the experiments on animals.

Now it appears the muscle has a nerve in addition to the motor nerve, which being necessary to its perfect function, equally deserves the name of muscular. This nerve however has no direct power over the muscle, but circuitously through the brain, and by exciting sensation it may become a cause of action.

Between the brain and the muscles there is a circle of nerves; one nerve conveys the influence from the brain to the muscle, another gives the sense of the condition of the muscle to the brain. If the circle be broken by the division of the motor nerve, motion ceases; if it be broken by the division of the other nerve, there is no longer a sense of the condition of the muscle, and therefore no regulation of its activity.*

* Thus led to conclude that there is motion in a circle, we nevertheless cannot adopt the hypothesis of circulating fluids. That a fluid does not proceed from the brain, we may learn from this; that on touching the end of a motor nerve which

We have noticed, that there is a plexus formed both on the nerves which convey the will to the muscles, and on the nerves which give the sense of the condition of the muscles. The reason of this I apprehend to be that the nerves must correspond with the muscles, and consequently with one another. If the motor nerve has to arrange the action of several muscles so as to produce a variety of motions, the combinations must be formed by the interchange of filaments among the nerves before they enter the muscles, as there is no connection between the muscles themselves. As the various combinations of the muscles have a relation with the motor nerves, the same relations must be established by those nerves which convey the impression of their combinations, and a similar plexus or interchange of filaments therefore characterizes both.

We have seen that the returning muscular nerves are associated with the nerves of sensibility to the skin, but they are probably very distinct in their endowments, since there is a great difference between conveying the sense of external impressions, and that of muscular action.

In surgical operations the fact is forced upon our attention, that the pain of cutting the skin is exquisite, compared with that of the muscles; but we must remember that pain is a modification of the endowment of a nerve, serving as a guard to the surface, and to the deeper parts consequently. This

has been some days separated from the brain, the muscle is excited as when the nerve was first divided. The property, however it may be defined, is therefore in the nerve. Our language might perhaps be made more precise if we used terms which implied the course of nervous influence, whether from or towards the brain; but it will be difficult to express this without the aid of hypothesis.

is further exemplified in the sensibility of the skin to heat; whilst, on the contrary, a muscle touched with a hot or cold sponge during an operation, gives no token of the change of temperature but by the degree of pain.

Many of the nerves which perform the most delicate operations in the œconomy, are not more sensible to pain than the common texture of the frame. The lower degree of sensibility to pain possessed by the muscles, and their insensibility to heat, is no argument against their having nerves which are alive to the most minute changes of action in their fibres.

When the anatomist shall find both the portio dura of the seventh and the fifth going to the integuments of the head and face, he may naturally ask, why are there two nerves to the surface? and he will probably reflect, that although the principal office of the nerves of the skin is to convey impression to the sensorium, yet the influence of the mind is conveyed to the surface. The condition of the mind in passion, for example, is as forcibly communicated to the skin as to the muscles themselves; and therefore if a branch of the fifth be necessary to convey sensation from the surface to the sensorium, the seventh is necessary to the change of vascular action, and to the condition of the pores when affected by a cause proceeding from within, outwards.

I feel a hesitation when I reason upon any other ground than on the facts of anatomy. Experiments are more apt to be misinterpreted; and the very circumstance of a motor and

sensitive nerve being generally combined together, affords a pregnant source of error.

It is natural to suppose that the galvanic influence might be brought to bear on this subject; but I may be permitted to suggest to any one who pursues it in this way, that it will be necessary to distinguish the effects produced by the nerve as a mere conductor, and when performing its living functions. The nerve, dead or alive, may convey the galvanic power like a wet cord; but if the nerve be in possession of its living property, a great deal will depend on the direction in which the galvanic fluid is transmitted. If it be transmitted against the course of the nervous influence, it will reach the muscles and act feebly, but the power of the nerve will not be exercised upon the muscles; but if it be transmitted in the proper course towards the muscles the nerve itself will be excited, and its power propagated so as to produce violent action in the corresponding muscles.

XIII. *On the constitution of the atmosphere.* *By* JOHN DALTON, *Esq. F. R. S. &c. Communicated January* 12, 1826.

Read February 24, 1826.

THE fact discovered by BOYLE and MARRIOTTE, that the space occupied by air is in the inverse ratio of the pressure, is one of great importance in the doctrine of elastic fluids. It may probably not be mathematically true in extreme cases; but in those where the condensations and rarefactions do not exceed 50 or 100 times, there is reason to believe the above ratio is a very near approximation to the truth.

Sir ISAAC NEWTON has shown in the 23d prop. book ii. of the Principia, that if homogeneous particles of matter were endued with a power of repulsion in the inverse ratio of their central distances, collectively they would form an elastic fluid agreeing with atmospheric air in its mechanical properties. He does not infer from this demonstration that elastic fluids must necessarily consist of such particles; and his argument requires that the repulsive power of each particle terminate, or very nearly so, in the adjacent particles. From the scholium to this proposition, NEWTON was evidently aware of the difficulty of conceiving how the repulsive action of such particles could terminate so abruptly as his supposition demands; but in order to show that such cases exist in nature, he finds a parallel one in magnetism.

Whatever we may think of the constitution of an elastic fluid, it is clear, that for the purpose of ascertaining its *mecha-*

nical effects, we may safely adopt the above hypothesis within the fore-mentioned limits of condensation and rarefaction.

The existence of elastic fluids totally different from atmospheric air in their chemical nature, but agreeing with it in mechanical properties, was unknown in NEWTON's time. Such fluids are now known to exist; they may be mixed together, and, in case no obvious chemical action ensues, they are found to occupy the same space after as before mixture, and in due time to be uniformly diffused through the space the mixture occupies, whatever may be the difference of their volumes and specific gravities: *and such mixtures have all the mechanical properties of simple elastic fluids*, as the atmosphere itself evinces, which is a mixture of at least four such elastic fluids.

Whether the uniform diffusion of elastic fluids through each other is occasioned by the repulsion of the elementary particles of the same kind, which appears to force them through most bodies, as well solid and liquid as aerial, except glass and the metals; or whether it is caused by attraction or chemical affinity, may be doubted. Difficulties attend both views. I have long been inclined to adopt the former notion as most consistent with the phenomena. According to this view the particles of any elastic fluid (A) are endued with repulsion to each other by the Newtonian law above stated; also those of any other (B) repel each other in like manner; but the particles of (A) do not repel those of (B), or rather are inelastic in regard to them. Such mixture must evidently possess the mechanical law of condensation which the atmosphere possesses, and which NEWTON's imaginary homogeneous fluid possesses.

All our ordinary experiments are limited to given volumes of gases which are considered of equal densities and temperatures throughout the volumes; but this is not the case when the volume is large, and extended in a direction perpendicular to the horizon; such, for instance, as a cylindrical column of the atmosphere of the altitude of several miles. The density in such vertical columns diminishes nearly in geometrical progression to equal intervals of ascent, and the temperature diminishes nearly in arithmetical progression, or in direct proportion to the ascent.

When we observe the diffusion of two gases (A) and (B) through each other in small limited volumes, such as in our ordinary experiments, we may ascribe it on the one hand to the mutual attraction of the particles of (A) and (B); or on the other, to the repulsion of the particles of (A) for each other, and their non-repulsion of those of (B); and *vice versâ*. The effect would seem to be precisely the same on both views. But it is not so when we consider a vertical atmospherical column of mixed gases. Here the two views display their difference in a striking manner.

An exposition of the results of two indefinitely long perpendicular columns of any given gases (A) and (B) being mixed together, having never yet been laid before the philosophical public according to either opinion, I apprehend it may be of service to the advancement of knowledge on this interesting subject to draw attention to the following considerations.

Let A and B be two equal cylindrical tubes placed in contact and perpendicular to the horizon, of indefinite length, closed at the bottom and open at the top. Let the tube A have

an atmosphere of hydrogen gas in it, so as that it would support the mercury in the barometer at 30 inches. Let the tube B have an atmosphere of carbonic acid gas also capable of supporting 30 inches of mercury. Now supposing it possible for the atmosphere to be for a few moments of uniform density throughout the column, and that density the same as at the surface of the earth, the height of the hydrogen atmosphere would be about 66 miles, and that of the carbonic acid about 3.3 miles; or the heights would be in the ratio of 20 to 1 nearly. Afterwards, when the atmospheres were expanded to their natural extent, equal elasticities of the two gases would also be at altitudes as 20 to 1; that is, if at two miles of elevation the carbonic acid atmosphere supported 15 inches of mercury, that of hydrogen would support the same at 40 miles elevation. These are positions too obvious to be insisted upon. Conceiving now the atmospheres to have acquired their perfect equilibrium, or to be at rest in the respective columns, let numerous air-tight horizontal partitions be imagined across both tubes at equal intervals from the ground upwards. These intervals may be either small, as a foot, or larger, as a mile, as may suit our purpose.

Let now a communication be opened between each two horizontal portions of the tubes, either by a perforation or a small tube, as represented in the annexed figure, at 1, 2, 3, 4, &c. Then it is well known that the two gases would intermix, and finally obtain such equilibrium, that *one half* of the gas at first in each division would pass into the opposite division, and the other half remain where it was. Hence, the whole weight of gases in each whole tube would be still the

same as before, namely, 30 inches of mercury; half of which in each tube would be carbonic acid, and the other half hydrogen.

Very great differences would be found in the proportions of the two gases in ascending, viewing them either in regard to volume or to weight. In the lowest division, or No. 1, we should find equal volumes of carbonic acid and hydrogen. At the height of two miles, we should find about one volume of carbonic acid mixed with two of hydrogen; at the height of four miles, the carbonic acid would be to the hydrogen as one to four, nearly; and at the height of 40 miles, there would probably be no carbonic acid at all in either tube, but the hydrogen would there be of one half the density it was in No. 1. Above this, or above the limits of the carbonic acid atmosphere, wherever it might be, there would be nothing but hydrogen gas in each tube up to the limits of the hydrogen atmosphere.

The limits of the atmosphere having been mentioned, it may be proper to observe, that on the hypothesis of the density of any atmosphere diminishing in geometrical progression to intervals of ascent in arithmetical progression, every atmosphere must be unlimited, or of infinite extent. But if any atmosphere is constituted of particles on the Newtonian hypothesis, it is obvious that such atmosphere must have a limit; this limit will exist where the repulsion of two particles becomes equal to the weight of one of them.

We have no data from which to determine the *absolute* height above the surface of the earth to which any one atmosphere can ascend; but we can form a pretty accurate comparison of the *relative* heights to which two atmospheres

would ascend, especially if the relative weights of their atoms be known.

For instance; we know that the diameter of an elastic particle of carbonic acid is nearly, or exactly, the same as that of a particle of hydrogen under the same pressure; also that their weights are as 20 to 1. At two miles elevation, the elasticity of an atmosphere of carbonic acid gas is diminished one half; and at 40 miles elevation, that of hydrogen is diminished one half. Now let it be supposed that at 30 miles elevation the carbonic acid atmosphere ceases to exist, or terminates, at which elevation its elasticity must be according to the geometrical progression, nearly $\frac{1}{33000}$; then, by the same law, the elasticity of the hydrogen atmosphere must be $\frac{1}{33000}$ at the height of $15 \times 40 = 600$ miles; also the diameters of the particles of the two gases are still equal at those elevations, because they vary as the cube roots of the elasticities inversely; that is, if the diameters of the particles of carbonic acid and hydrogen at the surface of the earth be denoted by 1, that of carbonic acid at 30 miles will be represented by $\sqrt[3]{33000}$, and that of hydrogen at 600 miles elevation will also be $\sqrt[3]{33000}$. But by hypothesis, this distance is capable of supporting a weight as 20 (namely the weight of one atom of carbonic acid); the hydrogen atmosphere therefore must be further elevated till it is capable of supporting a weight only as 1 (namely, the weight of an atom of hydrogen); this will take place when the elasticity is still further diminished in the ratio of the cube of 20 to the cube of 1, or 8000 to 1. Hence, we shall have to extend the atmosphere about $15 \times 40 = 520$ miles further before it can terminate, or to the height of 1120 miles. In this estimate we have not

taken into consideration the variable force of gravity. At the height of 1400 miles the force of gravity is reduced one half, nearly; on this account the elevation of the hydrogen atmosphere will be increased between 1 and 2 hundred miles more, so as to make it amount to twelve or thirteen hundred miles. The variation of temperature in ascending does not materially affect our views.

Thus it appears that upon the assumption we have made, the hydrogen atmosphere must be 40 times the altitude of the carbonic acid atmosphere. If we had assumed the utmost height of the carbonic acid atmosphere less than 30 miles, the disproportion of the two heights would have been still greater; and if more than 30 miles, it would have been less; but in this case the absolute difference would be greater.

If it be true that atmospheres have limits, or certain degrees of rarefaction beyond which they cannot be extended, it will produce certain modifications in the mixtures of the two gases in our tubes A and B, which will now require consideration.

Suppose the cell 1000 to be that at the summit of the hydrogen column A, or where the hydrogen atmosphere terminated before any communication was made with the column B. Then, on opening the communication of that cell with the corresponding one of B, one half of the gass would flow out as usual; but the two cells, instead of being filled with the dilated gas, would only be *half* filled with it. The gas would fill the lower half of each cell, pressing upon the lower partition, and the upper half would exist as a void. The same remark would apply, but in a less degree, to the inferior cells 999, 998, &c. and it would not be till a descent

of 40 miles that the gas of one cell would be adequate to fill two cells, making the density in each the same as that in No. 1000 before the communication. In all that interval of 40 miles the continuity of the atmosphere would be interrupted, each cell having a partial void, and the partition having a pressure on it from above, and none beneath. In like manner it might be shown that the carbonic acid atmosphere, were it alone, and subsequently made to communicate with empty cells, would be two miles below the summit of the atmosphere before one cell of gas could fill two cells.*

After a complete equilibrium of intercourse had taken place between every two adjacent cells, let us next conceive all the horizontal partitions to be withdrawn from the two tubes, and consider what results will ensue.

It is evident the descent of the upper part of the hydrogen column in each tube would be immediate, as there would be vacuous places to fill up in it. The same would take place with the carbonic acid column; but the great body or weight of the mixed atmospheres would remain unchanged, except a slight condensation. The column of hydrogen in each tube would support 15 inches of mercury, and would in all respects resemble the upper half of the first column, A, of hydrogen gas, that supported 30 inches, excepting a slight difference occasioned by distance from the earth and temperature; and the same may be observed of the carbonic acid column in each tube. But would this constitution of the mixture be

* Query, might not the absolute height of an atmosphere of carbonic acid gas (or any other) be found, by perfectly exhausting a tall receiver, then letting in a small given portion of the gas, and finding by some chemical test that the gas existed in the lower but not in the upper portion of the receiver?

permanent? Would a mixed atmosphere, which in fact as a whole, consisted of equal *weights* of carbonic acid and hydrogen, continue to exhibit at the surface of the earth equal *volumes* only in mixture? Or, on the other hand, would not the whole be wrought up in due time into one uniform composition in all its extent, of 20 volumes of hydrogen with one of carbonic acid, as many suppose to be the nature of the earth's atmosphere with regard to its component parts?

Before these questions are discussed we shall put the case in a different form: suppose a mixture of 20 volumes of hydrogen and one volume of carbonic acid (that is equal weights of each), were put into a large reservoir under the constant pressure of 30 inches of mercury, and from this reservoir were passed by means of a stop-cock into the indefinite perpendicular tube, A, perfectly void, till such time as the equilibrium between the reservoir and the tube was established: query, what would be the final arrangement of the two gases in the tube? I believe it will be allowed by all, that the final arrangement of the mixture of gases will be precisely the same in this case as in the one previously stated, whatever that arrangement may be.

Now I apprehend it is demonstrable, from what we know of the nature of mixed gases, that each of the two gases would be disposed just the same as if the other was not present. They would be mixed in equal volumes at the earth's surface; the carbonic acid would *rapidly* diminish in density in ascending, and terminate perhaps at 28 or 30 miles of elevation; the hydrogen would *slowly* diminish in density, and terminate perhaps at 11 or 12 hundred miles of elevation.

The arguments in support of this notion may be derived from the following facts:

1st. When two gases, not having a manifest chemical action upon each other, are put into a vessel of small limited capacity, they are found in a short time to be uniformly diffused through the capacity of the vessel, whatever be their proportions.

2d. Let a bottle, having its air exhausted, be half filled with water, and the other half with a mixture of equal volumes of two gases, suppose hydrogen and carbonic acid; then let an air-tight stopper be applied, and the contents of the bottle be duly agitated. The carbonic acid will be found equally diffused through the whole capacity of the bottle, the same in the water as out of it; but the hydrogen in the water will only be $\frac{1}{50}$ or $\frac{1}{60}$ of the density of that above the water. In this case each gas will be arranged, both within and without the water, precisely the same as if it was the only gas present. And if a third gas could afterwards be introduced into the bottle in like manner, it would take its place in and out of the water independently of the other two; and so on with any number. No pressure of any one gas on the surface of the water can confine another gas in the water; it must be a pressure arising from the same gas.

3d. If a portion of ether, alcohol, &c. be put into a bottle, and it be close corked, the vapour will ascend and fill the bottle, whatever air be present; its quantity and force will be the same whether there be any air, or none, being entirely regulated by the temperature.

From these three facts, but more especially from the two last, it appears to me as completely demonstrated as any

physical principle, that whenever two or more such gases or vapours as we have been describing are put together, either into a limited or unlimited space, they will finally be arranged each as if it occupied the whole space, and the others were not present; the nature of the fluids and gravitation being the only efficacious agents.

We may now apply this doctrine in considering the earth's atmosphere, on the supposition of its being in a quiescent state. The gases constituting it are azotic and oxygenous chiefly, a very small proportion of carbonic acid, and a small proportion of aqueous vapour. If we assume the weight of the atmosphere = 30 inches of mercury, and neglect the carbonic acid and aqueous vapour as inconsiderable in weight, we shall have $\frac{21}{100}$ of 30 = 6.3 inches for the weight of the oxygenous atmosphere, and $\frac{79}{100}$ of 30 = 23.7 inches for the weight of the azotic atmosphere. For the weights of the whole atmospheres on this view are proportional to the *volumes* found at the surface of the earth, and totally independent of their *specific gravities*. The weight of the aqueous vapour atmosphere is variable, and may be on an average = .4 of an inch of mercury, and that of carbonic acid = .03 of an inch of mercury.

The limit of altitude in a full atmosphere (of 30 inches mercury) of oxygen gas being assumed at 45 miles, that of an atmosphere of the same gas of 6.3 inches of mercury will be found by calculation on the above principles to be about 38 miles, the atom of oxygen being 7; and that of azotic gas of 23.7 inches weight will be found 54 miles, if the atom of azote be taken as 5; but if the atom of azote be double this weight, as is supposed by many, but I think without

sufficient reason, then the height of the azotic atmosphere will be only 44 miles. The very fine and attenuated carbonic acid atmosphere must ascend to the height of 10 miles, if a full atmosphere of this gas ascend to 30 miles; and that of steam or aqueous vapour to the height of 50 miles, allowing the specific gravity of steam to be .625, and the weight of its atom to be 8.

It may be worth while to contrast this view of the constitution of the atmosphere with the only other one, as far as I know, that has been entertained.

According to one view.

1. The volumes of each gas found at the surface of the earth are proportional to the whole weights of the respective atmospheres.

		Weight of atmosphere.
Azote -	$= \frac{79}{100} =$	79
Oxygen -	$= \frac{21}{100} =$	21
Aqueous vap.	$= \frac{1}{75} =$	1.33
Carbonic acid	$= \frac{1}{1000} =$	.10
		101.43

According to the other view.

1. The volume of each gas found at the surface of the earth *multiplied by its specific gravity*, is proportional to the whole weight of the respective atmosphere.

		sp. gr.	Weight of atmosphere.
Azote -	$= \frac{79}{100} \times$	.97 $=$	76.6
Oxygen	$= \frac{21}{100} \times$	1.11 $=$	23.3
Aque. va.	$= \frac{1}{75} \times$	.625 $=$	0.83
Car. acid	$= \frac{1}{1000} \times$	1.53 $=$	0.15
			100.88*

* In order to show the ratios more completely we have assumed the two compound atmospheres differing a little in the total weights.

According to one view.

2. The altitude of each atmosphere differs from that of every other, and the proportions of each in the compound atmosphere gradually vary in the ascent.

3. When two atmospheres are mixed, they take their places according to their specific gravity, not in separate strata, but intermixedly. There is however a separate stratum of the specifically lighter atmosphere at the summit over the other.

According to the other view.

2. The altitude of each atmosphere is the same, and the proportion of each in the compound atmosphere is the same at all elevations.

3. When two atmospheres are mixed, they continue so, without the heavier manifesting any disposition to separate and descend from the lighter.

All that we have said hitherto has been relating to *quiescent* atmospheres, or such as are in a state of perfect equilibrium. How the case would be with regard to the earth's atmosphere, such as it actually is, in a state of continual motion and agitation greater or less in all its parts, it is not very easy to ascertain; and it is besides rather a question to be decided by experiment and observation than by any theory. I have a series of observations made on this subject; but as they will require to be submitted in a considerable detail, I shall reserve them as a sequel to this essay on some future occasion.

1000
999
998
997
6
5
4
3
2
1
B
A